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DEPARTMENT OF ENERGY
AUTHORIZATION
(Magnetic Fusion Energy)**

HEARING
BEFORE THE
SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT
OF THE
COMMITTEE ON
SCIENCE, SPACE, AND TECHNOLOGY
HOUSE OF REPRESENTATIVES
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1990 DEPARTMENT OF ENERGY AUTHORIZATION

TUESDAY, FEBRUARY 21, 1988.

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
Washington, DC.

The Subcommittee met, pursuant to call, at 1:08 p.m., in room 2318, Rayburn House Office Building, Hon. Marilyn Lloyd (Chairman of the Subcommittee) presiding.

OPENING STATEMENT OF HON. MARILYN LLOYD, CHAIRPERSON, SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Mrs. LLOYD. The Subcommittee on Energy Research and Development will come to order.

Good afternoon, ladies and gentlemen. This is our first hearing on energy in the 101st Congress. While energy has not been on the front pages lately, it is of no less importance than it was in the late 1970s and the early 1980s.

If anything, energy research is more important for 4 reasons: First, the world population continues to expand thereby increasing energy demands; second, we continue to exhaust our natural energy resources; third, most of our energy supply degrades the environment; and fourth, imported oil continues to contribute to our trade deficit by \$35 billion per year.

We are therefore vitally concerned about moving ahead with the development of new resources which will provide clean energy at price levels that will support a strong economy.

As we proceed, we will be hearing from witnesses on solar energy, conservation, fossil energy, and nuclear. Today, we will receive information on fusion energy.

Our witness today from the Department of Energy will be Robert O. Hunter, who is Director of the Office of Energy Research. It is certainly good to have you with us, Dr. Hunter. As I said earlier, I enjoy working with your brother on the Armed Services Committee. He is certainly a hard-working Member of Congress, and you are a very hard-working representative of the Department of Energy, and we are looking forward to your testimony. Please proceed.

But, first of all, I would like to recognize my counterpart, the Honorable Sid Morrison, from the State of Washington.

OPENING STATEMENT OF HON. SID MORRISON, RANKING REPUBLICAN, SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Mr. MORRISON. Thank you, Madam Chairman. I'm certainly pleased that we are kicking off these hearings. We note from the schedule that you have us running through our entire responsibility in the budget rather quickly but very, very thoroughly. So we look forward to getting this started.

I'd like to welcome, on our side, Madam Chairman, Steve Schiff, from New Mexico.

Mrs. LLOYD. And Steve got off to a good start by attending field hearings, and I can see that he's going to be a very active and participating member of this committee.

Mr. SCHIFF. Thank you, Madam Chair. And I just have to say, though, that field hearing, as you know—you led the delegation—was in Illinois, and when Mr. Fawell found out I was born and raised in Chicago there was no way I was going to avoid going to Illinois for that hearing. [Laughter.]

Mr. MORRISON. I'm sure that Steve will be eager to move his way up here to the seat right next to you which, of course, replacing Manuel Lujan, who we wish every great success over in the Department of Interior.

The Magnetic Fusion program, which is today's specialty, has made some very good technical progress. It's bringing us one step closer to the possibility of controlled fusion. I'm also pleased to see that the conceptual design of the International Tokamak Experiment Reactor has successfully completed the first phase, because establishing a firm base for international collaboration is necessary for reducing fusion development cost and providing more clear-cut information for future operation of the Compact Ignition Tokamak.

I will submit the rest of my statement for the record, Madam Chairman. I think that as we look at the total energy picture I think it's important that we recognize the role that fusion can play. I know that from talking with you that your view toward energy policy has us counting on fusion after we get a few more decades down the road and the wise investment that we make now, and I think we'll be hearing about that investment as an absolutely essential part of America's energy future.

Mrs. LLOYD. Thank you very much.

[The prepared opening statement of Mr. Morrison follows:]

OPENING STATEMENT
HON. SID MORRISON
FY 1990 DOE AUTHORIZATION - MAGNETIC FUSION
FEBRUARY 21, 1989

GOOD AFTERNOON. I AM PLEASED TO JOIN MADAM CHAIRMAN AND MY COLLEAGUES IN OUR SUBCOMMITTEE'S FIRST HEARING ON DOE'S FISCAL 1990 BUDGET FOR CIVILIAN ENERGY R&D TODAY. I'D ALSO LIKE TO WELCOME OUR NEW MEMBER, CONGRESSMAN STEVE SCHIFF FROM NEW MEXICO. (ANOTHER NEW MEMBER PAUL HENRY FROM MICHIGAN IS IN HIS DISTRICT TODAY.)

THE MAGNETIC FUSION PROGRAM HAS MADE SOME GOOD TECHNICAL PROGRESS LAST YEAR, BRINGING US ONE STEP CLOSER TO THE POSSIBILITY OF CONTROLLED FUSION. I AM ALSO PLEASED TO SEE THAT THE CONCEPTUAL DESIGN OF THE INTERNATIONAL TOKAMAK EXPERIMENT REACTOR (ITER) HAS SUCCESSFULLY COMPLETED THE FIRST PHASE, BECAUSE ESTABLISHING A FIRM BASE FOR INTERNATIONAL COLLABORATION IS NECESSARY FOR REDUCING FUSION DEVELOPMENT COST AND PROVIDING MORE CLEAR-CUT INFORMATION FOR FUTURE OPERATION OF THE COMPACT IGNITION TOKAMAK, OR CIT.

THERE ARE DIFFERENT WAYS TO PURSUE BETTER UNDERSTANDING OF FUSION ENERGY CONFINEMENT IN PLASMA, AND TO PIN DOWN IGNITION THRESHOLDS. BUT WHEN THE BUDGET IS TIGHT, WE MAY HAVE TO LIMIT OUR INVESTMENT OPTIONS AND WORK HARDER TO ACCOMPLISH OUR GOALS IN A TIMELY MANNER. A CASE IN POINT IS THE CONSTRUCTION OF THE CIT. BETWEEN 1988 AND 1990, THE ESTIMATED COST HAS INCREASED \$100 MILLION (NEARLY 30%), AND THE ESTIMATED SCHEDULE FOR COMPLETION HAS SLIPPED MORE THAN 3 YEARS. IF

THIS TREND CONTINUES, WE MAY HAVE A DIFFICULT TIME MAINTAINING THE VIABILITY OF THIS PROGRAM.

I THINK FUSION CAN BE AN ALTERNATIVE TO CONVENTIONAL ENERGY SOURCES, AND CAN PLAY A SIGNIFICANT ROLE IN MITIGATION OF THE SO-CALLED GREENHOUSE EFFECT. I BELIEVE WE SHOULD CONTINUE TO IMPROVE OUR PLANNING AND ESTIMATING, AND BUILD A STRONG BASE FOR INTERNATIONAL COOPERATION, SO THAT FUSION ENERGY WILL INDEED BECOME A VIABLE ALTERNATIVE IN THE NEAR FUTURE.

I LOOK FORWARD TO HEARING FROM OUR DISTINGUISHED WITNESSES TODAY.

Mrs. LLOYD. And, Dr. Hunter, we look forward to hearing from you now.

**STATEMENT OF DR. ROBERT O. HUNTER, JR., PH.D., DIRECTOR,
OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF
ENERGY, WASHINGTON, DC**

Dr. HUNTER. Well, thank you. Madam Chairman and Members of the Committee, it is my pleasure to appear before you, the Committee, to discuss the Department of Energy's request for the FY 1990—

Mrs. LLOYD. Excuse me. I'm not sure your microphone's on or maybe whether it's close enough.

Dr. HUNTER. It has a light on.

Mrs. LLOYD. That doesn't mean a lot around here. [Laughter.]

Dr. HUNTER. Can you hear me better now?

Mr. MORRISON. It's better.

Dr. HUNTER. Okay. Thank you.

Madam Chairman and Members of the Committee, it is my pleasure to appear before the Committee to discuss the Department of Energy's request for the 1990 Magnetic Fusion program. Accompanying me today is Dr. Anne Davies, Acting Associate Director of the Office of Fusion Energy. I have submitted written testimony for the record. With your permission, I will proceed with some brief summary remarks.

Mrs. LLOYD. Without objection, you may summarize and your entire written statement will be made a part of the record.

Dr. HUNTER. Thank you. First and foremost, our main interest is the long-term application of fusion for providing energy. I have talked or corresponded with you already concerning a confinement physics initiative that is the object of a reprogramming. This initiative was taken to ensure the long-term health of fusion by providing physical understanding of the confinement process in Tokamaks. Without this we are concerned about the future of the program.

We are proceeding with the support of the whole program to focus on better confinement physics in tokamaks. Of course, different people and groups may have different approaches in this matter. There has already been work in this direction, notably on the TEXT device, and we are expecting good results from General Atomics.

In addition, the large program such as TFTR is coming on board with proposed experiments. It is a fruitful time for such an initiative because the diagnostics for the interior behavior of these machines are becoming available and we can make good progress. Also, the Magnetic Fusion Advisory Committee is involved and has formed a special subpanel. We don't, in fact, understand the fundamental mechanisms controlling how you hold the plasma in, and until we understand that we can't understand how it behaves in general.

I would like to point out that the confinement initiative is a prominent example of what we expect to be a dominant theme in the Department's approach to research and development; that is, competitive research and development. In a time of limited fiscal

resources and with clear problems to solve, this will be the best way to make rapid progress in fusion. In the simplest sense, competitive R&D means having 2 or more parties working on the same problem. The one who gets the right answer first is the winner, but, of course, we all win in terms of the understanding and the rate of the progress that we make. We have blocked out resources in the confinement initiative to be given to the parties with the best ideas.

You have asked about the pace for the CIT within the context of this initiative and how best to advance the field. When I took over this job, I was surprised to discover that people were talking about the CIT in terms of ignition probabilities, and, in fact, many of them talked about significant value from the CIT, even if it did not ignite. But CIT has been consistently proposed to you as an ignition machine and it remains true that the study of ignition physics is the primary reason for wanting to build the machine.

I became concerned that there was a significant probability we wouldn't accomplish goals set for the machine and have recently asked the Magnetic Fusion Advisory Committee to examine this question explicitly, and at the moment, their Confinement Physics Subpanel is looking into it. At present, my personal impression is that the probability that the proposed CIT will actually achieve its ignition goals may be unacceptably low.

The confinement physics initiative is aimed toward understanding the controlling issues in tokamak confinement with the specific objective of designing an affordable CIT that will achieve its ignition goals and, thereby, answer the critical questions for which we strongly feel this machine is needed. Ever since the Mirror machines were dropped from the Magnetic Fusion program the tokamak has been our main line of development. A workable and affordable CIT has been and remains the main goal of this development program. A CIT that is affordable may not be workable in a probabilistic sense, so our number one priority in confinement physics is to make a workable CIT.

As to the program structure, we are restructuring with this goal and priority in mind. This effort is in progress and the full Magnetic Fusion Advisory Committee has yet to review the work of its subpanel. Out of all of this we will develop new policy guidance for fusion to ensure that we do obtain a CIT with a certainty of ignition.

Thank you.

[The prepared statement of Dr. Hunter follows:]

Statement of Robert O. Hunter, Jr.

Director

Office of Energy Research

Department of Energy

before the

Subcommittee of Energy Research and Development

of the

House Science, Space and Technology Committee

February 21, 1989

Madam Chairman and Members of the Subcommittee:

I am pleased to be here today to discuss the FY 1990 budget request for the Magnetic Fusion Energy program. The continuing goal of the Magnetic Fusion Energy program is to establish the scientific and technological base required for an assessment of the feasibility of magnetic fusion as an energy source.

Fusion as a source of energy is attractive for several reasons. The fusion fuels (deuterium and tritium) are easily obtained and virtually inexhaustible. In addition, fusion reactors can be designed to yield very little long-lived radioactive waste. Finally, fusion powerplants would have outstanding safety features because there are only small amounts of relatively low-hazard fuel present at any one time and the fusion reaction could not run out of control.

The greatest challenge in producing fusion energy for practical use is to develop a device that can heat the deuterium-tritium fuel to the required high temperature and confine the heated fuel long enough so that more energy is released in the fusion reactions than is used to heat the fuel. The Tokamak Fusion Test Reactor (TFTR) goal is to confine the fuel at high temperatures long enough to achieve "breakeven" condition--where the fusion energy released is equal to that required to heat the fuel. Beyond breakeven, the next major step in the fusion program will be to achieve ignition, the condition in which the fusion reaction products take over the heating of the plasma and the plasma begins to burn without external heat sources. Once ignition has been achieved, the next goal will be a long pulse, or steady-state, burn and technology development.

The program called for by the Department's strategy, and further defined in the Magnetic Fusion Program Plan of 1985, includes work on each of four key technical issues: the properties of burning plasmas, magnetic confinement systems, fusion materials, and fusion nuclear technology. Although the Fusion program supports work aimed at addressing each of the key technical issues, the FY 1990 budget request gives highest priority to the first two. This will include strengthened experimental and theoretical efforts in high leverage areas of research on tokamak confinement and burning plasma physics. Improved understanding of tokamak confinement and burning plasma physics will help to advance the program more rapidly and is expected to reduce the cost of the development of fusion energy.

Let me take a moment to comment on the four key technical issues. The first of these concerns the properties of burning plasmas. No experimental facility exists anywhere in the world to investigate burning plasmas. The present Compact Ignition Tokamak (CIT) proposal is a two-phase experimental attempt to attain ignition in a modest-size device and to study and optimize fully ignited plasmas, should they be attained. The proposed device makes use of equipment and facilities at Princeton Plasma Physics Laboratory to reduce the cost of CIT. The existing design, which uses only existing TFTR power supplies, does not assure ignition. The probability of ignition, based on empirical global scaling laws for confinement, is reasonable, particularly with the ability to add additional power supplies if warranted. I have requested that a review be conducted to examine the prospects for ignition, including the need for additional power supplies. We will provide the results of this review, including implications for the budget, in the near future.

All the qualitative features known to improve confinement (high magnetic field, high current, high plasma density, discharge shaping, etc.) are exploited in the CIT device. There is, therefore, a consensus throughout the fusion community that a device of this general type should be built. Efforts are underway to increase the ultimate capability of the device and to better specify the need for any additional power supplies or heating systems. Thus, the general parameter region for the device is known, and necessary R&D on magnet design is proceeding. The FY 1990 budget request, which I will discuss in a moment, includes funds for both R&D and initiation of construction activities.

The second key technical issue concerns magnetic confinement systems. Significant progress continues to be made in understanding the processes involved in confinement. During the past year, improved confinement in tokamaks has been extended. New confinement conditions were achieved in TFTR at Princeton Plasma Physics Laboratory and DIII-D at General Atomics. Additional confinement improvements were observed in the Joint European Torus (JET), ASDEX in West Germany, and JT-60 in Japan (up to a factor of 3) compared to previous results. Nevertheless, there is a pressing need to improve our understanding of the physics mechanisms controlling energy confinement. To improve our ability to produce, heat, and control the requisite plasmas, the FY 1990 budget request provides for the application of recently developed experimental and theoretical capability to the physics mechanisms controlling energy confinement because this information is critical for planning for CIT and could reduce the size and cost of future steps in fusion development.

The third key issue concerns the identification and testing of materials for fusion systems. Achievement of the program goal will require development of new materials to reduce the induced radioactivity and, thereby, enhance the economic and environmental potential of fusion. Materials research is being pursued through cooperative international agreements that provide significant foreign contributions toward the operation of U.S. research facilities for mutual benefit. The FY 1990 budget request supports the core research program needed for U.S. participation in this international effort.

Advances in the basic engineering sciences will be required to address the fourth key technical issue, the nuclear technology of fusion systems. For this issue to be resolved, fuel-producing blankets must be integrated into other fusion systems and tested in a nuclear environment. These fusion blankets perform several functions, including converting the energy released by a fusion reaction into useful energy, creating the tritium fuel for the reactor, and allowing for recovery of this fuel, as well as shielding critical components from radiation. The FY 1990 budget request supports selected basic technology research that underlies the development of blankets for a fusion engineering test reactor.

Research carried out by the U.S. Magnetic Fusion program on the four key technical issues constitutes the basis for our participation in the world fusion program. This participation continues a long history of scientific exchange and cooperation. The most significant current international cooperative effort is that aimed at developing a single conceptual design for an International Thermonuclear Experimental Reactor (ITER). The ITER device

would, if built, be the follow-on to CIT that allows for the study of sustained ignition and burn physics. This device would also be used for the technology development necessary to lay the foundation for the design and construction of future devices. This effort, carried out under International Atomic Energy Agency auspices, includes the U.S., the USSR, Japan, and the European Community. After preliminary work and negotiation in 1987, the joint design effort began on April 21, 1988. The FY 1990 budget request provides for continuing U.S. participation in the conceptual design and validating R&D at the level of \$16 million dollars per year per party. Therefore, over the three years of the ITER effort, approximately \$192 million of effort will be devoted by the four parties to the preliminary design and supporting research and development. Once the design is complete, each participating country will have the information needed to make its own decisions regarding a future ITER-type facility.

FY 1990 MAGNETIC FUSION ENERGY BUDGET REQUEST

The FY 1990 budget request for Magnetic Fusion Energy is \$349.2 million. This request includes \$307.9 million for Operating Expenses, \$13.2 million for Capital Equipment, and \$28.1 million for Construction (Table 1). This budget request provides for high priority research based on increased use of existing tokamaks, application of additional diagnostic equipment to existing devices, increased efforts on development of a more attractive heating technique, and exploitation of innovative ideas for extending improved confinement of operation.

Table 1
Magnetic Fusion Energy
(\$ in millions)

	<u>FY 1988</u>	<u>FY 1989</u>	<u>FY 1990 Request</u>
Operating Expenses	\$295.7	\$316.7	\$307.9
Capital Equipment	18.7	21.6	13.2
Construction	<u>16.9</u>	<u>12.4</u>	<u>28.1</u>
Total	\$331.3	\$350.7	\$349.2

Let me now summarize the Operating Expenses request for each of the interrelated functional areas within the Magnetic Fusion Energy program: Confinement Systems, Applied Plasma Physics, Development and Technology, Planning and Projects, and Program Direction (Table 2).

The Confinement Systems subprogram, with an FY 1990 Operating Expenses request of \$168.1 million, addressed the two high priority technical issues for the Fusion program--improving magnetic confinement systems and determining the properties of burning plasmas. This research is focused on tokamaks and involves developing the data base needed to prepare for a burning plasma experiment and to identify an improved confinement system. The primary scientific issues being addressed by this research are energy confinement, heating, plasma stability, current control, and physics and particle control at the edge of a plasma.

In FY 1990, research will be conducted on several toroidal devices to investigate these physics issues and to prepare for performing the burning plasma physics experiments on TFTR and CIT. The confinement of high temperature plasmas will be studied on TFTR at the Princeton Plasma Physics Laboratory. Preparation for operation with deuterium-tritium fuel will be delayed to allow continued operation of TFTR to focus experiments on confinement in the world's hottest tokamak. To aid in the study of confinement physics, improved diagnostics will be installed for these experiments. The Operating Expenses request also includes \$15 million for R&D related to the CIT project.

Table 2
Magnetic Fusion Energy
Budget Authority
(\$ in millions)

	<u>FY 1988</u>	<u>FY 1989</u>	<u>FY 1990 Request</u>
Operating Expenses			
Confinement Systems	\$159.6	\$174.8	\$168.1
Applied Plasma Physics	74.7	78.2	73.2
Development and Technology	55.9	54.1	56.7
Planning and Projects	0.9	5.0	4.9
Program Direction	4.6	4.6	5.0
Subtotal	<u>\$295.7</u>	<u>\$316.7</u>	<u>\$307.9</u>
Capital Equipment	18.7	21.6	13.2
Construction			
Compact Ignition Tokamak	8.0	3.5	5.5
General Plant Projects	8.9	8.9	9.3
Confinement Physics Research			
Facility	0	0	13.3
Subtotal	<u>\$ 16.9</u>	<u>\$ 12.4</u>	<u>\$ 28.1</u>
Total	\$331.3	\$350.7	\$349.2

A number of medium-sized toroidal devices will be operated to help improve our understanding of how to produce, heat, and control the plasma required for fusion energy. Scientists at the DIII-D device at General Atomics have made steady progress in determining the upper limits of a parameter called "beta," which is the ratio of plasma energy to magnetic energy. In 1988, a world record beta of 6.8% was achieved. Scientists at DIII-D will continue to explore alternate plasma shapes and operating modes predicted to increase beta, while also extending understanding of confinement obtained through magnetic field shaping.

Fabrication of the Alcator-C Modification at the Massachusetts Institute of Technology (MIT) will be completed in FY 1990. This experiment will test various aspects of the CIT design, physics assumptions, and projected operating modes. The Microwave Tokamak Experiment (MTX), at Lawrence Livermore National Laboratory, which was fabricated using the old Alcator-C device from MIT, began operating in FY 1989. It provides the unique capability to test the application of pulsed high power microwaves as a more efficient heating technique in tokamak devices.

Two devices are currently operating to study advanced toroidal issues. These devices are the Advanced Toroidal Facility (ATF) at the Oak Ridge National Laboratory and the Princeton Beta Experiment (PBX) at the Princeton Plasma Physics Laboratory (PPPL). These devices are conducting experiments to determine the existence of a second stability regime and determine the plasma physics requirements for operating with high performance and improved stability. The Advanced Toroidal Facility, which is a stellarator type device, is also looking at the issues of stability, transport, heating and

particle control in this type of device. As the program continues to focus on tokamak confinement physics in FY 1990, operation of ATF will continue, but on a half-time basis. The PBX facility, although valuable in the long run, will be mothballed in FY 1990 while resources at PPPL are focused on TFTR and CIT.

International collaboration will be relied on to carry out research on a number of related plasma issues in foreign facilities, including TEXTOR and ASDEX in West Germany, Tore Supra in France, the JET in Europe, and JT-60 in Japan. In each of these cases, the U.S. provides technical personnel and specialized equipment in return for participation in the planning and conduct of the research and sharing of the results.

The Applied Plasma Physics (APP) subprogram, with a FY 1990 budget request of \$73.2 million, plays a major role in addressing the magnetic confinement and burning plasma issues at a very fundamental level. The subprogram conducts research on basic magnetic confinement physics, develops information on new diagnostic techniques and techniques to heat and control plasma, and provides computational support for the overall program.

In FY 1990, APP will continue to expand our understanding of magnetically confined plasmas through experiments with alternative magnetic configurations, including tokamaks, compact toroids, and reversed field configuration devices. These configurations provide different physical environments in which magnetically confined plasmas obey the same overall physics laws governing plasma stability as well as transport of energy and particles within the plasma. The Texas Experimental Tokamak (TEXT) at the University of Texas provides a center for investigating the basic physics of the transport of

particles and energy within plasma and for developing diagnostics. Other, small, university plasma devices test new ideas on current drive and heating or test specific theoretical predictions. Research with non-tokamak configurations includes: (1) design of a major reversed field device, the first concept to be tested in the Confinement Physics Research Facility at the Los Alamos National Laboratory; (2) operation of a new reversed field device at the University of Wisconsin to investigate edge physics issues in reversed field configurations; (3) completion and first operation of a field reversed device at Spectra Technology with supporting work at the University of Washington; and (4) spheromak physics research at the University of Maryland and at the University of California Berkeley.

In FY 1990, theoretical research will refine predictions of alpha particle physics in burning plasma, continue to develop predictive capability for radio frequency heating, and emphasize predictions and modeling of processes controlling the transport of energy and particles in toroidal plasma. Large scale computational needs are provided through the operation of the Energy Sciences computing network.

The Development and Technology subprogram, with a budget request of \$56.7 million, develops components and systems for use in near-term and mid-term fusion experiments, and conducts research and development on long-term generic materials and fusion nuclear technology. This subprogram also performs conceptual design studies for future fusion devices. These studies identify component and system development needs that must be met to prepare for the eventual application of fusion. The international collaborative program, ITER, is funded from this subprogram.

In FY 1990, this subprogram will continue to pursue research on technologies needed to form, contain, heat and sustain a reacting fusion plasma. Development of higher field capability superconducting magnets will be continued to help reduce the plasma physics needs and reduce the size and cost of fusion reactors. The heating and fueling systems developed to date have enabled the production of record plasma conditions in fusion devices, and this U.S. technology is valued highly in the development of agreements for collaborative work internationally. Fueling the plasma with pellet injectors, which are funded from this program, is instrumental in achieving density profiles in plasmas which produce better confinement. The heating systems developed in the technology program are being used to achieve temperatures in present devices and are being improved to do the same in larger fusion devices.

Materials research will continue to develop low-activation materials that will reduce the need for long-term waste disposal and limit materials damage due to the bombardment of neutrons and plasma inside the fusion reactor. In this regard, Oak Ridge National Laboratory achieved significant progress in 1988 in the development of a reduced activation structural alloy for the ITER design. Technologies needed for various blanket concepts will also be investigated for their ability to perform the multiple functions of heat extraction, tritium production, and radiation shielding. Finally, design and R&D work will continue in support of ITER and in particular on the power handling and particle control issues.

The Planning and Projects subprogram primarily provides support for construction of major new facilities and fabrication of auxiliary components for these facilities. The FY 1990 Operating Expenses request of \$4.9 million is primarily for compliance with requirements associated with the Small Business Innovation Research program.

The Operating Expenses request of \$5.0 million for Program Direction provides for personnel and other costs associated with continuation of 62 full-time equivalent staff.

The FY 1990 Capital Equipment request of \$13.2 million will provide for essential hardware to support the overall program. This equipment includes diagnostic and computer equipment, power supplies, and other components for experimental facilities.

The FY 1990 Construction request of \$28.1 million provides \$13.3 million for the Confinement Physics Research Facility (CPRF) at Los Alamos National Laboratory. CPRF is being proposed as a line-item construction project for the first time in FY 1990. The project was funded as an Operating Expense project through FY 1989. With a total estimated cost (TEC) of \$75.6 million and an estimated completion date of FY 1993, the CPRF will bring to the program an experimental capability to explore the physics properties of a reversed field toroidal confinement experiment that has the theoretical capability, in a future device, of heating the plasma to ignition with lower magnetic fields. The FY 1990 request will support fabrication of the coils and vessel/shell.

The FY 1990 Construction request also includes \$5.5 million for the Compact Ignition Tokamak. The project, with a TEC of \$455.0 million, began in FY 1988 and is scheduled for completion in 1996. FY 1990 activity in support of the CIT will include detailed design of CIT systems and conventional facilities, long lead procurements for tokamak components, and initiation of site preparations.

The remainder of the Construction request, \$9.3 million, is for General Plant Projects (GPP). These GPP projects at Los Alamos National Laboratory, Princeton Plasma Physics Laboratory, and Oak Ridge National Laboratory provide for the continuing minor alterations and modifications necessary to protect the government's investment in its facilities.

That concludes my statement. I would be happy to answer questions.

DISCUSSION

Mrs. LLOYD. Thank you very much, Dr. Hunter.

I'd like to go back to the issue of reprogramming. It seems to me that this has sort of changed the direction of our fusion program. Now are you planning further redirections in this program beyond the new policy guidelines or is that what you're referring to?

Dr. HUNTER. Until we have the results of the full Advisory Committee review of the effort, we do not have at this time any further redirection to propose. So it would only be something that would flow from that review process. At the moment, in the proposed budget, the reprogramming, as it was stated is the full extent of any redirection in the program.

Mrs. LLOYD. What will the budget be, in this Administration's budget?

Dr. HUNTER. I believe the budget is about \$349 million for FY 1990.

Mrs. LLOYD. How is this divided up? Do you have very much information at this time?

Dr. HUNTER. Yes, ma'am. In operating expenses, it is just over \$300 million, \$308 million. In construction, it's \$28 million. In capital equipment, \$13 million. For a total of \$349 million.

Within the operating expenses, the largest category are confinement systems at \$168 million, and the other two substantial categories are applied plasma physics, \$73 million, and development and technology, \$56 million.

Mrs. LLOYD. The 28 million for construction, what is this to—

Dr. HUNTER. That includes \$5.5 million for the CIT construction, \$13.3 million for the Confinement Physics Research Facility construction at Los Alamos, and \$9.3 million in general plant and program construction.

Mrs. LLOYD. Do you think we're spreading ourself a little bit too thin at this time? Are we trying to achieve the difficult task of achieving ignition with too little funding?

Dr. HUNTER. Ma'am, my views about that are that the key here is the need for scientific information still in this program. You have invested historically very substantial funds in a variety of machines and facilities in this country, and I think that with the new availability within the last 1 to 2 years of diagnostics that can actually look inside the machine, what's going on in the interior plasma, you're in the position here to make a great deal of scientific progress very quickly. I think your concern about the possibility of monies being spent to support, say, facilities that we've built in the past and things of that nature and spreading us too thin is a very real one, but I think that we have a very substantial opportunity before us and that the funding levels we have, as proposed to you are adequate to fully exploit that opportunity.

Mrs. LLOYD. Well certainly it's the goal of this committee and this Congress to have some major focal points and to really show the American people what we have done with the tax dollars in this direction. At the same time I think it's also pertinent that we look at how we can attract other nations to participate in our programs. Are we making significant efforts in this regard?

Dr. HUNTER. Well, I would ask Dr. Davies to speak to you in detail about that. But I would say the fusion program is a model for international cooperation and collaboration. It has been since the late 1950s and, in fact, has served as a model for many of the other collaborations ongoing today in science and technology, and I believe that we have made every effort, and are continuing to, to be involved internationally and hence make the best use of our resources.

Mrs. LLOYD. I was specifically referring to CIT because we have had international cooperation for many years.

Dr. HUNTER. With CIT?

Mrs. LLOYD. Yes, sir.

Dr. HUNTER. Well, we've had cooperation in CIT specifically in the people reviewing the program 2 years ago. We, the program itself, as formulated, is largely a program funded by the United States to be taken in concert, from an international viewpoint, with other machines funded by other countries building towards the ITER activity for a test reactor development as an international activity. But the activity itself has largely, I believe, been seen and characterized as a U.S.-supported effort.

Mrs. LLOYD. Dr. Davies, would you like to respond to this?

Dr. DAVIES. That's correct, Madam Chairman, that CIT is seen as a U.S. contribution to the world fusion program. However, we have begun to talk with our international partners about the possibility of their actually participating in the construction and then in the operation of CIT, and there is some interest there around the world.

Mrs. LLOYD. So efforts have been made in this regard.

Dr. DAVIES. We have begun to talk to people. I don't believe we can seriously negotiate with people until we begin construction activities ourselves and have a firm commitment to move forward with CIT ourselves. But there is interest. The review people, the people from other countries who came to the review last year were very supportive and have continued to be very supportive of the CIT project and program.

Mrs. LLOYD. How can we get other nations more interested in our project?

Dr. DAVIES. I think we will have to have a firm commitment from the United States to move forward with it, and then we can negotiate seriously with potential partners.

Mrs. LLOYD. And that is the point I was getting to.

What are the major constraints in achieving ignition? Is it confinement? Or stability?

Dr. HUNTER. Well, I would say that there are varying views of that, but I think the engineering is relatively straightforward. It may be massive, but the engineering base that has been developed in fusion is more than adequate to handle it.

The major issue is that the CIT is in a parameter space outside of that in which we've built machines before and it represents a very large investment. And it's the certainty with which you can extrapolate the laws that we know in the regions where we've worked, called scaling laws, if you will, to regions outside of where they have previously been made to work. And that's where the emphasis on the understanding, from a physical basis, what controls

confinement, so that you can with confidence extrapolate to another physical region, that's where the confinement initiative is involved. And I think the major issue with regard to ignition is what the energy confinement time is going to be in the geometric and operational parameter regimes of the machine.

Mrs. LLOYD. One final question. Do you feel that the anticipated funding level for CIT represents a large enough investment to qualify this as an investment that the international community should be considering?

Dr. HUNTER. I don't fully understand that question, Madam Chairman.

Mrs. LLOYD. Are we doing enough this year in funding the CIT program to encourage international concern and consideration?

Dr. HUNTER. Well, I think we certainly are doing enough to encourage their consideration, and I feel—

Mrs. LLOYD. And involvement.

Dr. HUNTER [continuing]. That arriving at a very high—and we will make sure that we do all we can to encourage their involvement further. I think that arriving at as certain a basis as we can for ensuring the machine will work as advertised will be the single most effective means we can introduce, as well as a construction start, if you will, as a definite point in time for encouraging their involvement and participation.

Mrs. LLOYD. Thank you.

Mr. Morrison?

Mr. MORRISON. Thank you. I noticed—I recall something from before. There was a 5-year plan—

Dr. HUNTER. Yes, sir.

Mr. MORRISON [continuing]. That was in place. Are we on track with that plan, or have we deviated from it?

Dr. HUNTER. Well, let me ask Dr. Davies to comment in detail on that. I believe that with the proposed construction start on CIT you have not yet deviated substantially from that plan. In the matter of when D-T is burned in TFTR, that may well have deviated from the plan significantly. But let me ask Dr. Davies to comment specifically.

Dr. DAVIES. The reprogramming that the Department has proposed for fiscal year '89 carries over into our proposal for '90, so we made changes in our 5-year plan of a year ago, at least for fiscal year '90, to continue this emphasis on transport understanding in tokamaks into 1990. So, if you compare in detail, it's a similar set of changes to the '89 reprogramming.

Mr. MORRISON. How about the mothballing of the PBX?

Dr. DAVIES. That was included in our plan a year ago and we still plan to mothball PBX beginning in 1990.

Mr. MORRISON. Okay. I guess I ask about this only because one of the criticisms we've gotten from the appropriations folks who make some of the measurements there's a lot of things that we've started in the scientific arena that have not played all the way out to successful conclusion. Not the fault of this subcommittee, but primarily the fault of a shortage of funds, and we get—as priorities change, we get cut off.

I want to pursue the international thing because I think what you're sharing with us is one of the real international success sto-

ries as far as shared science is concerned. I notice our share is, of the total mix is going down so we must be successfully attracting others to invest. And are those investments coordinated so that we're efficiently using everyone's funds?

Dr. DAVIES. We are making a very concerted effort to do that. We're doing it through our bilateral agreements with the Japanese and the Europeans in particular, and also through our collaboration with the Europeans and the Japanese, both, through the International Energy Agency. Especially in the areas of long range technology programs, we're making an effort to do the planning and the defining of what's required early on together. That lays the groundwork for then preparing and proposing joint experimental programs and sharing the efforts and the cost around the world.

I doubt that we will ever get to the point that we're 100 percent efficient in the world fusion program, but we're making every effort to work toward that and we've had good results in working with the Japanese and the Europeans. The International Tokamak Experimental Reactor that you mentioned earlier is a prime example where we in the United States believe something of that size should be shared, the cost and the work should be shared, and we have made great efforts there also to make that collaboration, at least in the design phase, a success, and it is working well.

Mr. MORRISON. I need two predictions. One, when will that successful ignition occur? And secondly, when will we be ready to go commercial? With that you can have your money. [Laughter.]

Mr. MORRISON. We need some guesses on time, just out of interest. We have to play the same game also.

Dr. HUNTER. You want an estimate as to when you will see ignition in the CIT machine?

Mr. MORRISON. Or however we get to it.

Dr. HUNTER. Right.

Mr. MORRISON. You had some questions about CIT.

Dr. HUNTER. From my viewpoint, the shortest way to get to ignition is to spend 2 years working hard on making sure we understand the physical basis of the energy confinement with ongoing R&D activities or things of a like nature related to CIT, and then we foresee a construction period on the order of, I believe, some 6 years to actually build the device. And so the machines typically come into operation within roughly 2 to 3 years after the end of construction. So I think that, if you add those numbers up, you obtain a time period in the range of 10 to 15 years, in that range. Hopefully, in the time frame of roughly 10 years. And I think that, within 1 to 2 years, we should be able to tell you that the probability of ignition of that machine is a high and relatively firm number. So I would offer you that.

The period of commercialization is something that's very speculative. The first consideration being that the CIT itself, which is a high field machine and high density, is not necessarily directed towards an optimal reactor. So what you have left to balance are the enormous strides in investment and technology made in the fission arena, where people have built, as you know, in excess of 100 reactors over the last 30 to 40 years and developed a great deal of technology in this country and many more worldwide, versus how ap-

plicable that technology is to the fusion problem, which is similar. It's nuclear engineering but it has some twists particularly its own.

And I would add as a third consideration that I think the demand in particular, the amount of—the willingness to spend the amounts of engineering funds required will then become just a dominant issue as to how fast you can go to commercialization. And there again I think this energy confinement issue is major.

TFTR, for example, if the machine were working in what's called in a neoclassical regime, or as well as you would expect to without any anomalous things happening to you, is a machine that's near ignition in terms of size and complexity. So, if you can make design machines, if your physics confinement understanding is such that you can design machines that are the equivalent of, say, a few hundred megawatt plant, as opposed to today they would be the equivalent of a several thousand megawatt plant, very large with enormous capital requirements in a single plant, then I would foresee a relatively short period of commercialization, and if there were very substantial demand, a period of, perhaps, as low as, you know, a few tens of years, if you will.

If, on the other hand, what you're left with are machines that are going to be at the thousands of megawatt electrical class and, say, require a capital investment of on the order of \$10 billion, then both the engineering and I think the speed at which you could commercialize such a machine, you know, become hindering factors in wanting to go very fast.

So I hate to leave you such an open range of answers, but the fact remains that if the fusion, in terms of the plasma behavior, obtains an innovation or two that lets us run near the theoretical limit, I think that leads you to a fairly short path to both ignition, the monies required for that, and it leads you to smaller plants where you can, hopefully, acquire the bulk of the engineering already developed, to see its eventual commercial application.

If we do not obtain those kinds of results and end up deciding that the only things we know how to build are machines that are at the several thousand megawatt class with enormous capital requirements, then I think you could be facing time periods where you—you know, periods of time where you are driven just by necessity, perhaps, to fusion on some grounds, and that could easily be 50 years or longer, in my opinion.

So I view the area very much today as dependent on the innovativeness and the invention of the parties working in the field, and I would qualify my answer in those bounds. If the community works hard and makes those breakthroughs, then I think you're left with a relatively short and direct development path.

Mr. MORRISON. Well, we would like to provide you with the wherewithal to pursue this goal enthusiastically, and I'm going to put you down at 2030 for commercialization.

Dr. HUNTER. I, you can put me down for that number. I do not recall having made that explicitly. [Laughter.]

Mr. MORRISON. You didn't. Thank you, Madam Chairman.

Mrs. LLOYD. One thing about it, we probably won't be here to challenge your testimony today. [Laughter.]

Mrs. LLOYD. Mr. Schiff?

Mr. SCHIFF. Thank you, Madam Chairman.

Dr. Hunter and Dr. Davies, I think I should start by introducing myself because I'm the new kid on the block here. My name is Steven Schiff. I represent New Mexico's First District, which was held for 20 years by now Secretary Manuel Lujan, Jr.

I have to say at first I can't resist saying again, though Mrs. Lloyd has heard it before, I'm a lawyer by profession and training. My brother has a Ph.D. in plasma physics. He thinks it's absolutely hilarious that I'm on this committee. [Laughter.]

Mr. SCHIFF. When his R&D budget comes up, he might not think it's so hilarious. [Laughter.]

It doesn't go through this subcommittee.

But I, the first couple of questions I'd like to ask may certainly be novice questions. I know you could go on all day in answer to this question, but in sort of 25 words or less can you kind of summarize the scientific community's view of how practical is it that we actually will have controlled fusion for energy? In other words, are we off the wall? Does it look pretty logical in terms of the steps that can be taken to arrive there?

I mean, where are we at? How far are we from theory into getting anywhere in practice?

Dr. HUNTER. My summary would be that the jury is out with regard to the scientific community. That, in fact, fusion has gone on for some time. It has made enormous scientific strides. It does not yet have a firm application or realization in an energy sense. And so I feel that they would basically be neutral towards it, relative to its competitors which may be advanced nuclear reactors, for example. And that you would not find a preponderance of opinion that said it was not going to make a very strong competitor, nor one that would say that it necessarily would.

And furthermore, I think you would see a realization on their parts that the environmental issues that we see developing are a major determinant and discriminant in such a competition. In other words, the amount of waste that fusion might produce. But I believe you would find them mixed on that question.

Mr. SCHIFF. Could I stay on that for a moment? Again, I know fusion—we have nuclear reactors. We don't have controlled fusion yet. Is there an expectation on the waste output of a fusion reactor if something were to exist, especially as compared with the nuclear reactors?

Dr. HUNTER. Yes, there are. There have been detailed studies done. We would be happy to provide them to you. The major point of interest is that the products of the fission reaction themselves remain as high level waste in a fission reactor, and with lifetimes well in excess of, typically, 100 years. The fusion materials can be tailored since the products of the fusion reaction themselves do not have, do not present real difficulties in terms of waste handling. You're looking at activation of the structure primarily. The fusion materials can be tailored to provide basically a medium level of waste, if you will, a much lower level of activity, of which the major component dies off much more quickly, in less than 100 years, as compared to a fission system.

So, in terms of the lifetime of its high level, you know, the lifetime of the products, of the bulk of its waste, you can make some-

thing that, on paper, appears very substantially advantageous, relative to a fission reactor.

Mr. SCHIFF. So the expectation would be less of a waste problem than under current nuclear engineering, is that right?

Dr. HUNTER. Yes. The expectation is that you could make it in—you could engineer a system that would have less of a waste problem, but I would also say the Department has invested a great deal of energy into the storing, into looking at how to store conventional radioactive waste. So, in the end, I think what you're looking at is a dollar and cents comparison: what it costs to store those high level wastes for the appropriate periods of time, as opposed to what it will cost to store the relatively high volume but somewhat lower level fusion wastes that you might obtain.

So it has this advantage, but I have not seen it quantified and carried through the entire waste cycle.

[The information follows:]

FUSION/FISSION COMPARISON

The potential attractiveness of nuclear fusion as an energy source relative to nuclear fission may be strongly characterized by our ability to select materials for fusion reactor systems that can substantially reduce the long-term burden of radioactive waste management. In contrast, the production of high-level radioactive waste in the form of fission products and actinide elements is intrinsic to the nuclear fusion process.

The management of radioactive waste must consider the intensity of radiation, the time dependence of that activity and the mass of material that must be managed. Current licensing requirements for land disposal of radioactive waste are specified in the Nuclear Regulatory Commission (NRC) regulations at 10 C.F.R. Part 61. Using these regulations, we have examined the potential of fusion relative to fission. The total life cycle mass of radioactive material is not much different for fission and fusion. However, fusion reactors may be designed and constructed from materials which generate no "high-level" waste that would require "deep geologic" storage under the NRC regulations. Further, analysis indicated that material options may be developed for fusion such that intruder access control would be no longer required after a time of approximately 100 years, and materials recycle then would become a possibility.

Mr. SCHIFF. Dr. Hunter, in the materials I have read it's indicated that fusion is expected to rely upon the fusion of deuterium and tritium, if I'm pronouncing both of those correctly. Deuterium it was said is found in seawater, virtually. It's a common element. But don't we manufacture tritium? I mean, in other words, if we had fusion and we were using tritium, wouldn't it be hampered by how well we could produce tritium? Or am I not correct in how we get tritium?

Dr. HUNTER. In the deuterium-tritium versions, which are the ones that are most strongly under consideration right now, the tritium in the reactor would be produced by part of the output from the fusion machine itself. In other words, when you make tritium today, you basically take neutrons and bombard lithium, and that makes tritium as a product. And in the case of fusion you have, for example, a lithium blanket around the fusion device and the neutron energy that comes off from the reaction, part of it is converted to tritium. So it produces its own tritium, if you will, and that's kept into account in the energy balance. So it breeds its own fuel, the tritium part that's put back into the fusion reactor.

Mr. SCHIFF. But tritium is man-made, is it not? It doesn't—

Dr. HUNTER. It's made naturally as well. It's made by cosmic rays and things of that nature. It just has a short half-life, 12.6 years, so the concentration around us is quite low.

Mr. SCHIFF. Okay. On the budget, Dr. Hunter, you stated several figures, but I need a relation. The figures that—I think you were quoting Administration proposed figures, I do believe.

Dr. HUNTER. Yes.

Mr. SCHIFF. Excuse me. Are they, where do they stand with respect to current fiscal year funding for fusion research, up or down?

Dr. HUNTER. Very close. The amount last year was, I believe, \$350 million point—\$350.7 million. This year it's \$349.2 million. So in terms of the dollar level, they're almost identical and there's not too much shift among the different categories that I gave to you.

Mr. SCHIFF. But, in other words, it's a proposal that I guess we're going to be hearing a lot of, of basically the same dollar figure without regard to inflation. Is that correct?

Mr. MORRISON. That's called a flexible freeze.

Mr. SCHIFF. And we're in the flexible part—we're in the freeze part of the flexible freeze, I gather. Is that correct?

Dr. HUNTER. I have no comment on that. [Laughter.]

Mr. SCHIFF. I'm sorry. Let me go on then.

What, you know, my experience is that workers desire to be paid more and materials go up in cost and so forth, so on any project, no matter what it is, if you're going to spend the exact same number of dollars, won't that in some fashion hamper the project during the next fiscal year if that's what the budget is? And if so, would you expect it to hamper the fusion research?

Dr. HUNTER. Well, this program—and I would ask Dr. Davies to comment in detail—since the early 1980s has decreased in excess of 50 percent in real dollars over that period of time. In other words, it has seen a considerable lessening of the total resources applied against it. It has at the same time maintained good technical progress in the course of that program.

It is true that there will be, that you would expect salaries and the scientific personnel to cost more. So in a real sense it represents, perhaps, a slight change from the previous year, the proposed budget does. But it is a program that has coped quite well and produced a very good output in terms of per dollars spent on its investment through the course, over that whole resource level, if you will, of spending during that time. So I think that the budget you have before you, you know, reflects our emphasis on wanting to go forward with some new initiatives in the machines, particularly related to CIT, CPRF and others. The area has matured substantially in that same period of time. A fair amount of understanding has been obtained. And so I would say you are seeing a budget that essentially is close to being flat in real—then-year dollars. In 1988, the budget was \$331 million. So, you know, you could say well there was a change between then and '89 that was significant, you know, and averaged over those 3 years, it looks like a reasonable increase.

But, in any case, it's been put together with the particular projects in mind, the particular areas in which the money would be spent, and that's, you know, what it consists of.

Mr. SCHIFF. In your opinion, are the budget figures quoted sufficient to continue the progress which you state has been made in this research?

Dr. HUNTER. Yes, sir. I think the levels are entirely adequate to proceed into what we view as the number one priority, this energy confinement; and prepare us to build the next large, you know, single large generation of machine in the program, and to carry on a variety of good experiments, particularly since we see this improvement in the diagnostic capabilities, as far as looking inside the machines is concerned.

The comment I would make to you is that you can build many machines, and they get to be expensive data points once they get to be large machines, or you can try to understand in detail what's going on in the machines. Right now historically I think you're seeing diagnostics and techniques for looking inside the machines that are recently developing on a rapid time scale. Within the last year we've seen our first real measurement in one of these areas, and I think those will provide us, at low cost, with a great deal of scientific progress.

Mr. SCHIFF. Just one more area, please, Dr. Hunter, I'd like to ask you about, international cooperation. In the area of fusion research is there any or an extensive amount of classified research, at least to the extent we would not be willing to share with our allies, Western Europe and Japan and so forth? Are we concerned about security of the information particularly?

Dr. HUNTER. No. In the magnetic fusion energy area which we're discussing here today, as far as I know there is no material that is classified or that we have restricted from discussion with other parties, and that does not form a barrier, if you will, to the full utilization of the international assets.

Mr. SCHIFF. So there ought to be an open road or open door, or whatever metaphor is appropriate, towards international cooperation on this subject; is that correct?

Dr. HUNTER. Yes, sir. And again, historically, in the late 1950s in particular, the fusion program was one of the very first and major avenues in which links were established at the scientific level between this country and the Soviet Union, and it has followed since with many other countries. That it has served as a model for that type of interchange.

Mr. SCHIFF. Representative Morrison mentioned a moment ago about, or the Chairwoman did, about materials we have that show the total percentage of investment in this research has been going up, especially by the Europeans compared to ourselves. Does that reflect our spending going down or being too flat, or does it reflect a great investment in those countries in this very same research?

Dr. HUNTER. Let me speak to the numbers as I asked to have them generated. We looked 10 years ago as to the fractions that were being spent by the different parties, and I believe at that time the U.S. was spending roughly half of the international, of the total monies being spent. In magnetic fusion the U.S. was spending roughly half. Today that number is roughly a quarter, and in real dollars the amounts of money have stayed constant between that 10-year-old snapshot and today. So I think that you've seen is that the international parties have picked up a substantial fraction of

the program and it is running at essentially the same level as it was 10 years ago.

Let me ask Dr. Davies if I've got that correct.

Ms. DAVIES. I believe that's correct.

Dr. HUNTER. Okay.

Mr. SCHIFF. But their investment, then, has gone way—I'm looking at the change in the percentages, however. Does that mean that the European figures have gone way up in terms of what they're spending?

Dr. HUNTER. You're now beyond what I can readily recall, but I believe the European Community numbers have gone up substantially in that period of time and the Japanese some. But primarily the European Community involvement and the Japanese involvement have made up the difference.

Mr. SCHIFF. One last thing. In terms of their cooperation, I've been informed that the Europeans are telling us that unless we get our own CIT work, continue on schedule, that we may not produce relevant research information for them because of where they are. In other words, they need the information they're planning to receive from us at a certain point in time in terms of the machines they're developing. Do you know if that's correct?

Dr. HUNTER. Well, I would say that the major machine which came about as a result of a summit initiative a few years ago, is the machine called ITER. We are, I believe, in the lead on doing the initial design and some engineering on that machine, and it will proceed through 1990. There is no commitment beyond that on how to proceed precisely. So with regard to information feeding into that machine, I think it's a question of what types of information you hope to have. And certainly that opinion is a very reasonable opinion, but I do not know of any major efforts that are going on outside the ITER context for a very large-scale reactor development effort.

Mr. SCHIFF. Well, is the ITER schedule waiting for our CIT information in order to—

Dr. HUNTER. I don't believe so. Let me ask Dr. Davies to comment. I think you'd find a varied opinion among the different European parties as to how applicable the CIT is to ITER even, since, as I explained to you, the CIT is a machine that is not in the parameter space that is a true reactor class of machine; whereas ITER is meant to be a forerunner of a true reactor. So let me ask Dr. Davies to comment in detail.

Dr. DAVIES. I believe the view of the international fusion community is that CIT would make a major contribution to how you would operate ITER. People would be very pleased to have seen an ignited plasma produced and studied well in advance of the initial operation of ITER. But I don't believe the world believes they should wait for CIT to operate in order to proceed with something like ITER.

The Europeans and also the Japanese and to some extent the Soviets do have efforts in addition to ITER. They're proceeding with parallel designs for ITER class devices, should they decide to go it alone, and those devices, the design for those devices look more and more like ITER as they work on the design. But with regard to the Europeans in particular, I don't believe the Europeans will proceed

with ITER or with their own, certainly not with their own NET. That's what I should have said there. I don't believe they'll proceed with their own ITER-class device until they've completed their large tokamak experiment, the JET—the Joint European Torus—and I believe they're just now in the process of considering extending the schedule for that experiment another 2 to 4 years.

Mr. SCHIFF. Thank you very much, Dr. Hunter and Dr. Davies. And, Madam Chairwoman, thank you for the indulgence you gave me and time for all those questions.

Mrs. LLOYD. Thank you very much, Mr. Schiff.

Mr. Walgren?

Mr. WALGREN. Thank you, Madam Chair.

This is a difficult area and particularly because of what's happened to other budgets in energy research and development. I represent Pittsburgh area, and, of course, we're very interested in coal, in fossil fuel. Fossil energy research and development over these years has been really eviscerated in the squeeze of the '80s. If my recollection is right, I think we were spending something in the range of \$800 million as we went into the '80s, and now the Administration's recommendation is \$163 million. That would be a—without considering inflation, a reduction of somewhere in the range of 80 percent, and then if you factored in the inflation effort on that it's hard to think we could be doing more than about 10 percent under that budget as proposed by the Administration.

When other budgets are being cut so deeply, how do we work up the level of effort that we can recommend for an alternative like magnetic fusion? Obviously, you're dealing in tremendous uncertainties. But, obviously, a critical benefit to mankind if it's possible to do it.

But can you tell me anything about how you relate the needs this year in magnetic fusion to the emphasis that ends up in a request to cut the other budgets by, in fossil fuels level, \$200 million in order to level fund this area which has such a high degree of uncertainty?

Dr. HUNTER. Well, the fossil fuel area is not direct familiarity to me, but I would make a couple of comments. I believe that, with the clean coal activities discussed, at this Administration is putting substantially more monies into that area than what you've indicated, and I'll be happy to obtain the precise numbers.

[The information follows:]

Clean coal technology budget authority—fiscal year 1990

	<i>Millions</i>
Reagan.....	\$325
Bush.....	710

The clean coal technology program is a government-industry co-funded effort to demonstrate advanced concepts for using coal cleanly and more efficiently than today's technology. President Bush will honor the commitment to the Canadians to devote \$2.5 billion in Federal funds over five years to this program. For FY 1990 the President proposes \$710.0 million, an increase of 274 percent over FY 1989. The budget also proposes that the remaining funds be provided in 1991 and 1992, consistent with previous commitments.

There were efforts, particularly very early in the 1980s, that were basically generated during the 1970s, to try to affect the market price, if you will, immediately for various synthetic fuels

and things of that nature involving coal. President Reagan's Administration I feel, you know, felt that those were inappropriate in terms of their market place involvement, and that the best way to handle the energy situation was to not try to interfere in that particular fashion in the marketplace, and so there were very substantial decreases in the funding applied to those programs at that time. Those were essentially carried out independently of the Magnetic Fusion Program, although both had received some impetus as a result. Certainly fusion, in 1973, in the energy crisis there had seen the impetus for its major growth emerge.

So I think that there were these historical efforts that had to do with, if you look at where you are in the marketplace, that led to those types of decisions. Today, with magnetic fusion, what we see is a situation where there is a long-term effort of a highly scientific nature involved which is beyond the immediate pull of the marketplace, if you will, and I think the rest of the world has similar reasoning behind it, and we're proceeding, you know, on those grounds.

The Department has carried out a number of initiatives and efforts, particularly in clean coal and methods of burning coal, to greatly reduce the SO_2 emissions, the NO_2 emissions, and NO_x emissions NO_x , over the last period of years. And I think that they feel that those efforts are commensurate with the improved equipment you could then hope to see put on coal-burning facilities. In other words, to leave the marketplace with an economic choice—

Mr. WALGREN. Yes.

Dr. HUNTER [continuing]. If we're to retrofit and still be able to meet emission standards.

Mr. WALGREN. Yes. Well, let me ask you this. Even in the military side there is a lot of talk about stretching out weapons procurement systems and a whole range of military activities which, obviously, are central to the national effort. Are we in a position to stretch out any of this magnetic fusion work? Does the Department work out a stretched out budget in any way?

Dr. HUNTER. I'm not certain of the question that you're asking. We formulate policy and programs in magnetic fusion and then the resources are cast against trying to, you know, obtain goals associated with those programs. Right now the major goal we have been working on and continue for is something called Q equals 1.

Mr. WALGREN. OMB does not come back to you and say, "We agree with the goal and we agree with the task necessary to get there, but we feel we ought to only go one quarter of the way this year as opposed to attempting to go all the way?"

Dr. HUNTER. Well, there is a series of questions asked by all of the various agencies that are involved in setting the budget and then their priorities. I mean, OMB is vigilant in understanding the amounts of money that have been spent in this program and wanting to make sure that they are appropriately spent in the future and you're making good progress towards your goals. But the goals here are long-term goals that would not immediately affect the marketplace, if you will. So they're set within a longer term research context, which requires some stability of funding, as opposed to a purely market-driven type of decision process.

Mr. WALGREN. You indicated in your testimony that you're initiating construction, a certain amount of construction activities, and then your tables break it out at \$28 million, \$13 million for the confinement physics research facility and the balance for 2 other subjects. When you say initiation of construction, how complete are we in those projects as to the construction end of it?

Dr. HUNTER. Well, I'll let Dr. Davies comment in detail. Again, in the request, the FY 1990 request, there were 3 major items under construction. One was the Compact Ignition Tokamak where we have been spending money for some 3 years in initial design and engineering, and the monies requested are to actually begin construction. It's a project that is some \$450 million and many years in length. So it's the beginning of a major effort.

Mr. WALGREN. When do we anticipate finishing spending that 400 million.

Dr. HUNTER. In 1996.

Mr. WALGREN. Ninety-six.

Dr. HUNTER. In the case of the CPRF, which is a particular machine—excuse the acronym—at Los Alamos. That program was carried out in a different fashion and we have chosen to make it a construction item this year, but some monies have already been expended related to it. And the other ones are basically a variety of somewhat smaller projects, plant improvement, if you will.

So we really only have one major construction activity; that is, the CIT, that we're just starting into. Historically, this program has spent a substantial fraction of its funds on construction projects. You have to build new machines to see how well they work. It's how it makes progress.

So that's a description, I guess, of the status of these different elements, and let me ask Dr. Davies to comment on those.

Dr. DAVIES. Perhaps one piece of information. We've committed about half of the cost, the estimated cost, for the CPRF. This is a reverse field pinch experiment being constructed at Los Alamos. It was funded as an operating expense project in the past, but in submitting our '90 budget we literally proposed it as a construction project. But it is not a new project.

Mr. WALGREN. Why is that? What would account for the change there?

Dr. DAVIES. It makes, it is a different format for presentation in the budget. It allows people who wish to track it very carefully to see the out-year projections, and to see the dollars specifically for that project each year.

Mr. WALGREN. Um-hum. Let me ask one other thought, and that is that it would seem that as international efforts increase, and particularly, as you say, they have apparently occupied maybe fully up to another third that they did not before of the total world effort, it would seem that we ought to be making a greater effort in exchanging information with the international community and that that should be able to be tracked in visits and travel expense of people who ought to be going to international conferences and bringing our best contribution to the world effort to bear. Sometimes you see people say that the wrong people go to conferences internationally; that oftentimes on a very limited budget we will send a bureaucratic head as opposed to a working theoretical scien-

tist who can actually engage and make a contribution to those conferences.

How do you try to be sure we send the right people to the conferences internationally, and how do you be sure that you have enough in your travel budget to support the increased effort in international exchange which we obviously should be making?

Dr. HUNTER. Let me ask Dr. Davies to comment on those detailed questions as well as she could to briefly describe to you the number and scope of agreements that we have internationally.

Dr. DAVIES. You were referring primarily to conferences, but our international collaboration is much more extensive than to conferences abroad. But with regard to conferences, we plan, for a year, the international conferences with more than just a few Americans planning to go, and then we very carefully try to plan how many people from which institution, who is going to give which paper, who is going to represent which experiment, who is going to bring back which information from other, from foreign experiments. So we try very carefully to plan for international conferences.

But, as I said, that's a small part of our international collaboration. We have extensive agreements in place where we exchange people, sometimes we take hardware, sometimes other, people from other countries bring hardware here. We have——

Mr. WALGREN. Do we budget that separately, so that somebody could see whether the effort at international exchange was increasing?

Dr. DAVIES. We can track it. We do not budget it separately. We budget at each laboratory for people's salaries and some amount of money for travel. Our major contractors include that in their proposals to us each year, and then they track it very carefully through their planning and through their expenditures each year. So that we can track it. We do not provide funding to a given contractor specifically to participate in a foreign experiment unless this is a small contractor, perhaps a university program with only a few people where travel abroad would be a major fraction of their annual budget. But ordinarily, we do it within the context of a contractor's annual budget.

Mr. WALGREN. Well, it's, you know, just a distant concern. But I think it probably warrants underscoring that as budgets get tight we all know that some of the wrong things go, and one of the things that's easiest to drop in governmental efforts are, particularly, international exchanges, and I would hope that you would have in the Department of Energy a very close tracking of what the effort in that area is because as we get out of the business in many ways and in something like this we are sort of wishfully relying on foreign developments and foreign—and our ability to participate with them. And then if we found that simply because it was politically the easiest thing to go, found ourselves foregoing that as well, I think we would all wind up disappointed. So I would just like to underscore that element in your internal decision-making.

Dr. DAVIES. I agree with that, if I could comment. The U.S. fusion program has gone the other way. As our budgets have become more constrained, we have more of an effort to work with international partners. We see it as a very real way to extend our domestic resources. We send hardware and teams of people to ex-

periments abroad when these are experiments that are unique in the world. It's something that, perhaps, we would have liked to have built but did not see how to do it. We go abroad with equipment, with people. We do it through negotiated agreements. We participate in the decision-making about the planning of experiments as well as in the actual experiments. We found it very beneficial.

Mr. WALGREN. There's a reverse side of that mirror, too. Not only when they are doing something which we're interested in shouldn't we be sending people who we would really like to see get a very close involvement with that, but also when we're not doing something—let's see, what is the reverse side of the mirror? It would seem there would be instances where we may have a partial accomplishment in this country which we really want to push out there and which we really want them to know about because of what we're not doing in terms of completion.

Dr. DAVIES. Well, we have also had good experience in that regard, also. Especially with the Japanese, who brought a team of people and a fair amount of money, \$70 million over a 5-year period, to the Doublet III experiment at General Atomics, and right now the Japanese are cost-sharing with us and participating in experiments on irradiation of materials and handling of tritium systems in this country.

Mr. WALGREN. Thank you very much, Madam Chairman.

Mrs. LLOYD. Thank you very much, Doug. That point is very well taken. I think CIT is a case in point where we have had a lot of interaction with the international community. But there again we need to make a firmer commitment to attract international interest and their cooperation with these programs. So it is a circle, what we're trying to do.

One of the points I'd like to bring up, you commented on using the machines that we have, Dr. Hunter. Why have we then limited ATF funding to that which will only provide for 6 months?

Dr. HUNTER. Well, the particular machine, ATF, is not a tokamak; it's a stellarator.

Mrs. LLOYD. That's right.

Dr. HUNTER. And the stellarators were originally developed in this country, and, then, in the wake of the Soviet invention of the tokamak and direct demonstration that it worked quite well in the late 1960s, in 1969 in particular, essentially the stellarators here were converted into tokamaks, and we then built a very large number of tokamaks. Tokamaks today represent, in the world, far and away the class of best performing machines.

Today, we find ourselves in a situation where we view that the confinement physics in tokamaks is the main driving issue before us, and ATF is a stellarator. There is some applicability of its physics, conceivably, to a tokamak confinement, but it's much less direct than actually measuring what's going on in a tokamak. So we have looked at the various machines—and I could ask Dr. Davies to comment—with regard to what types of measurements you can do on them and how applicable they are to tokamak confinement physics. And ATF, as a stellarator, is simply a less direct contributor to that knowledge that you're looking for than a tokamak directly.

Mrs. LLOYD. Isn't this a pretty short leash for our people at the laboratory to operate under?

Dr. HUNTER. No. Again, Dr. Davies can comment on it. We've spoke about this issue of competitive R&D. We want ideas, no matter from which group they come from, as to how best to quantify this confinement and measure it, and we expect to see those people, you know, involved in that. And I believe they are, in fact, engaged in that at this moment on tokamaks.

So let me ask Anne to comment.

Dr. DAVIES. Yes, our people at Oak Ridge, who were working on ATF full time, planning to, are beginning to work also with GA on Doublet III and with the University of Texas at TEXT, and, even though this reduces our effort on stellarator research in this country, it is a focusing on tokamak research.

I should mention that last summer when the magnetic fusion community met in, sort of in a retreat and talked about what we should be planning for the future of the program, what came naturally and forcefully out of that meeting was that we should be making an effort now to understand confinement in tokamaks. It is, what came of that meeting was an establishing a task force, a transport task force made up of experimentalists and theoreticians from throughout the U.S. program, to look at what we needed to do to better understand transport or confinement in tokamaks, and that group is going to be giving us a report within a matter of a month or so. Our MFAC panel is also going to be assessing what we should be doing about understanding confinement better. We'll have a final report from our advisory committee in June.

And the people from Oak Ridge have looked and are looking at what they can do in ATF to contribute to understanding tokamak confinement, and it will get due hearing from both of these groups.

Mrs. LLOYD. Very good. We'll do that with the next panel.

One final question. You commented on, in another area on the scaling relationships. The Europeans, it's my understanding, are confident that they know the scaling relationships to build an ignition reactor. Why don't we have confidence in these relationships?

Dr. HUNTER. Excuse me, ma'am. The Europeans in the sense of the ITER activity, a separate European activity?

Mrs. LLOYD. The JET. The JET community.

Dr. HUNTER. In the JET community—

Mrs. LLOYD. You mentioned in your testimony that the understanding of the scaling relationships, get a better understanding before we could proceed further.

Dr. HUNTER. Well, in the discussions I've had, and I'll ask Dr. Davies to comment also. I had the discussions with a head of the JET effort earlier this year about this topic and he did not indicate that to me at the time. I may well have misunderstood what he was saying.

In JET, they had expected performance, very good performance somewhat earlier and they have done quite well. But, again, I think that in terms of extension well outside the regime they're already operating in, in other words, in trying to make, if you will, a more compact reactor, I don't believe there would be a wide difference in their understanding and our own of these different types of scaling relationships.

Mrs. LLOYD. Dr. Davies?

Dr. DAVIES. Madam Chairman, I believe that is correct. We have extended international collaboration to trying to understand confinement also, and there have been meetings of experts internationally to try to assess what we do know and how we can extrapolate to something like ITER, and we share the uncertainties with our international partners.

Mrs. LLOYD. Thank you very much. Appreciate your testimony. We will be having—I'd like to state that although we are going through our budget hearings on a little more rapid pace than we usually do, we will be holding hearings throughout the year on the status and the progress of the technologies. Thank you very much.

Dr. HUNTER. Thank you, ma'am. We greatly appreciate the opportunity to come before you.

Mrs. LLOYD. Your testimony was excellent. We appreciate Dr. Davies and Dr. Hunter for being with us today.

Before we move on to our next panel, we have a new addition to the subcommittee. Frank Murray is now our staff director. Stand up, Frank. We're happy to have you. He is replacing Jack Dugan. We have a great staff on the Subcommittee. I think we're more of a nonpartisan staff than most of the committees, and I'd like to feel that we can accomplish more for the good of our country in this manner. Kitty Rising does an excellent job. We're proud of her. Ron Williams is with us. And Nelson is with us. We also have Kathryn Holmes and Kathy Smith, from our staff, who is with us. I think most of you know Rob Ketcham, of the full Committee, who has been around about as long or longer than I have, that we can count on. I hope that most of you in the audience know the staff members, but if you don't, I hope you will get to know them because certainly we're dependent on a good staff in this Subcommittee.

Thank you very much for being with us.

Dr. HUNTER. Thank you, ma'am.

Mrs. LLOYD. Now we will hear from the innovative ones. Our first panel will include Dr. Lee Berry, from the Oak Ridge National Laboratory; Dr. Harold Furth, from the Princeton Plasma Physics Laboratory; Dr. Rulon Linford, from Los Alamos; Dr. David Overskei, from GA Technologies; and Dr. Ron Parker, from MIT.

Gentlemen, we welcome you. It's always good to see you again, part of the Energy Subcommittee family. We look forward to seeing you every year.

I saw one of our usual witnesses at Argonne last week, and he said, "Hey, you didn't call on me to testify this year." I said, "Well, we'll have to check on that because we do look forward to seeing so many of you every year, and you certainly contribute so much to the energy program of our country and to our hearings."

Lee, I believe you're first. Go ahead.

PANEL 1

**STATEMENT OF DR. LEE A. BERRY, ASSOCIATE DIRECTOR,
FUSION ENERGY DIVISION, OAK RIDGE NATIONAL LABORATORY,
OAK RIDGE, TN**

Dr. BERRY. Thank you, Madam Chairman, Members of the Subcommittee.

Is this all right? I don't hear myself very well.

Mrs. LLOYD. From here you sound pretty good.

Dr. BERRY. Okay. Thank you.

Before looking ahead to the coming program, I would like to review some of the major accomplishments in fusion research over the past year. It's all too easy to focus on the technical and programmatic problems we face and lose track of the real progress being made towards the realization of fusion power.

Beginning first with Oak Ridge technology, improved low-activation alloys have been developed, and a tritium proof-of-principle pellet injector was built and successfully tested. The pellet injector testing was done in collaboration with Los Alamos on their tritium test facility, the TSTA.

Now to the plasma physics. Record numbers of neutrons have been produced at the TFTR at Princeton, and high-pressure plasmas with betas of over 5 percent have been maintained for long pulses on DIII-D facility at General Atomics. We are pleased to have ORNL staff members participating in both those experiments.

The successful operation at Oak Ridge of the Advanced Toroidal Facility in the second stability regime is a fusion advancement of which we're particularly proud. The ATF is the world's largest and most versatile stellarator. It was completed in late 1987 within budgeted cost and has now been operating for about a year. Installation of the diagnostic instruments needed to carry out the scientific program is still under way, although at a reduced pace due to reductions in the FY 1989 budget. I'll address the impact of these reductions in more detail later in my testimony, but first I want to discuss what ATF is and what it's for.

The magnetic fields that confine a plasma in a stellarator are similar to those in a tokamak but are produced in a different manner. The fields in a tokamak are produced by a combination of currents flowing in external coils and currents flowing in the plasma itself. The external currents can be controlled by the experimenter, but the plasma currents are difficult to control and hard to predict and measure. In a stellarator all the fields are produced by external currents; thus, we can accurately predict the magnetic configuration of a stellarator and systematically vary its parameters. In addition, because the fields are externally produced there is no need for a transformer and the stellarator is intrinsically a steady-state device.

The ATF design did not result from the evolution of a particular line of stellarators like the Wendelstein series of devices in Germany or the Heliotrons in Japan. Instead, it was developed by the tokamak group at Oak Ridge in response to a request by the Office of Fusion Energy "...to identify a facility with the goal of investiga-

tion of improvement in the toroidal confinement concept with emphasis on the principles of high beta, steady-state operation."

We believe that the design of ATF meets this challenge and that the research program will lead to improvements in the toroidal concept through two avenues:

First, by comparing the similarities and differences of confinement in ATF and in the conventional tokamak, we will be able to evaluate the various theoretical transport models, because magnetic configuration parameters enter these models in different ways for the 2 different concepts. The absence of a plasma current in ATF and our ability to vary its configuration will be invaluable in finding the correct set of models. This understanding will contribute to developing the predictive capability that we need to design economical fusion devices in the future.

Second, ATF is exploring the physics of the so-called second stability regime, which has been proposed for improved tokamak operation. To oversimplify, a conventional tokamak becomes less stable as the plasma pressure increases, and ultimately a pressure or beta limit is reached. However, calculations suggest the existence of a second stability regime. ATF can operate in this regime where stability actually improves as plasma pressure increases and much higher pressures can be maintained. Moreover, some models suggest that improved energy confinement will result from the reduced levels of plasma turbulence expected in this second stability regime.

Initial ATF data suggest improved stability with increasing plasma pressure and are consistent with theoretical calculations which indicate that ATF is, in fact, in the second stability regime. These results bode well for the future success of ATF and will help guide research on tokamak operation in this improved second stability regime.

We most often think of the optimum toroidal concept as being an advanced tokamak. If that is the case, then ATF will, we believe, as I've outlined above, contribute to the development of that tokamak. However, the stellarator might just be that optimum concept. As I indicated earlier, the stellarator is intrinsically steady state and has the potential for high beta and good energy confinement. ATF will go a long way towards validating those expectations. Further confirmation will come from a new Japanese stellarator, the LHS, or Large Helical System. This three-quarter-billion-dollar facility will be built at the new fusion institute at Toki near Nagoya and will be completed in 1995. A strong and effective ATF program is key to our effectively building on this three quarters of a billion dollar Japanese investment.

Our ability to exploit the capabilities in ATF and build on the promising initial results is being severely limited by the 1989 budget. It will be compromised even more at the proposed level for FY 1990. Total ATF funding in FY 1988 was a little over \$18 million. This is close to the roughly \$19 million per year which DOE presented to Congress about a year ago in its 5-year Magnetic Fusion Funding Plan which included building the compact ignition tokamak. Nineteen million, while not sufficient to allow full exploitation of ATF's long-term potential for steady-state operation,

would allow timely and efficient use of the over \$100 million investment in the device and its support facilities.

The initial FY 1989 funding for ATF operations was almost \$18 million, but was subsequently reduced to just under \$15 million. Operations had to be substantially decreased, and as a result experimental operating time will fall by about 50 percent. It also required us to defer and/or reduce the scope of the ion cyclotron heating, divertor, and neutral beam improvement tasks. Fabrication and installation of diagnostic instrumentation is proceeding, but utilization of these important tools will be slowed because of staff reductions and restricted operations.

The proposed budget of just under 14 million for FY 1990 would stop almost all diagnostic fabrication and installation and would require further substantial reductions in staffing. Timely and effective contributions to the confinement initiative will be extraordinarily difficult, if not impossible. We hope that additional funding can be provided so that ATF will continue the productive and valuable research program which is just now starting.

Implicit in our assumption that increased understanding will lead to improved plasma performance is the availability of the magnetic, heating, fueling, and heat removal components required to produce the conditions that the physicists say are needed for this better mousetrap. Historically, major parameter advances have resulted as much or more from new technology as from new physics insight. High fields on Alcator-C, neutral beams and carbon limiters on PLT, pellet fueling on Alcator-C and D-III, and now computer-based fabrication techniques on ATF are major examples of the technology enabling the physics.

This technology base is the infrastructure of fusion and must not be neglected even as the community turns more towards physics. We at Oak Ridge are proud of the role we have had in developing the technology that is now in use on devices throughout the world and giving us an in-road to the physics that these large and expensive devices can provide. Recent experiments on the Joint European Torus have utilized a pellet injector which was built at Oak Ridge to make major advances in the parameters of radio-frequency-heated plasmas. Scientists from Princeton and ORNL have participated in these experiments in England under the auspices of a U.S./Euratom collaboration on plasma fueling. Oak Ridge is also continuing collaborations in pellet fueling and RF heating with a large number of labs in the U.S. and abroad.

A milestone in international collaboration was reached this year with the formal conclusion of the International Energy Agency's large coil task. The participants in that effort have been asked by the IEA to look for ways to build on the experience and facilities of that program to further develop superconducting magnets for fusion applications. This is difficult in the light of an almost 50 percent reduction for funding for magnetics work at ORNL during FY 1989 and the proposed zeroing of the effort in FY 1990.

While discussing infrastructure, I believe it is important to note the contributions that fusion technology makes to U.S. industrial involvement. The major reason that the Japanese put forward for the development of fusion is the use of that technology in their industrial development. High-frequency microwave technology devel-

oped at Oak Ridge is now being used in the development of improved ceramics and in the development of improved plasma sources for semiconductor processing. Similarly, the understanding gained from the development of materials to withstand the fusion environment has provided the basis for improved high-temperature steels which will lead to more efficient electric power production. Active collaboration with industry is an essential part of these tasks, and I'd like to compliment the chairman on her legislative efforts to facilitate those interactions.

As I indicated earlier, progress is being made on the development of materials for fusion reactors. Previous work at ORNL showed that modifications to conventional stainless steels produced materials that were acceptable for use in first-generation fusion reactors. More recently, new alloys have been developed that would result in a thousandfold reduction in the radioactivity of the waste from a fusion reactor. Deep burial of this waste is not required, at least under present regulations.

In addition to these structural alloys, so-called special materials such as carbon-carbon composites, copper alloys, and ceramics are now being evaluated. These materials have important applications in next-generation engineering and physics test facilities as well as in demonstration and power-producing reactors.

As with any energy technology, fusion's ability to pass the test of economic and commercial viability will depend on materials. In fusion, the last test of public acceptance may well also depend on materials development.

ORNL's ability to meet both the materials need of next-generation engineering test facilities, and to carry out the long-range development needed for economical and environmentally attractive fusion reactors has been compromised by budget reductions in FY 1989. Proposed reductions in FY 1990 will further distort this balance. Similarly, FY 1989 reductions in support for RF technology and the need to keep commitment to support present facilities are reducing our ability to do long-term development. We hope that funds can be added to these areas to maintain a balance between short-term and long-term development.

The next-generation devices I discussed earlier are represented by the CIT and iter, led by Princeton and Livermore, respectively. ORNL is actively engaged in the physics and technology development of these projects and, with the Fusion Engineering Design Center, is contributing to the engineering. In addition, we are collaborating with UCLA on the ARIES advanced reactor study. It is through these studies that the potential of fusion is best seen.

The price of this progress is a better understanding of the problems that remain to be solved. The broad range of physics, technology, engineering, and ultimately economic and environmental issues we face is certainly formidable. However, we must remember that fusion is one of the very few energy options available to us in the long run. Our response to the understanding of a greater technical challenge must be the maintenance of a perspective on how far we have come and a rededicated attack on the problems that remain.

[The prepared statement of Dr. Berry follows:]

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Testimony to the
Subcommittee of the
House Science, Space and Technology Committee*

February 21, 1989

By
Lee A. Berry
Associate Director for
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Fusion Energy Division
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Oak Ridge, Tennessee 37831-8071

Madam Chairman and members of the subcommittee. It is a privilege to present testimony on behalf of my fusion colleagues at Oak Ridge. In looking back at the program over the past year, it is all too easy to focus on technical and programmatic problems and lose track of the real progress being made towards the realization of fusion power.

Beginning first with Oak Ridge technology, improved, "low-activation" alloys have been developed, and a tritium "proof of principle" pellet injector was built and successfully tested. The pellet injector testing was done in collaboration with Los Alamos on their tritium test facility, the TSTA.

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*Operated by Martin Marietta Energy Systems, Incorporated under Contract DE-AC05-84OR21400 at Oak Ridge National Laboratory.

developing the predictive capability that we need to design economical fusion devices in the future.

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However, calculations suggest the existence of a second stability regime. ATF can operate in this regime, where stability actually improves as plasma pressure increases and much higher pressures can be maintained. Moreover, some models suggest that improved energy confinement will result from the reduced levels of plasma turbulence expected in the second stability regime.

Initial ATF data suggest improved stability with increasing plasma pressure and are consistent with theoretical calculations which indicate that ATF is, in fact, in the second stability regime. These results bode well for the future success of ATF and will help guide research on tokamak operation in the second stability regime.

We most often think of the optimum toroidal concept as being an advanced tokamak. If that is the case, then ATF will, we believe, contribute to the development of that tokamak. However, the stellarator just might be the optimum concept. As I indicated earlier, the stellarator is intrinsically steady state and has the potential for high beta and good energy confinement. ATF will go a long way towards validating these expectations. Further confirmation will come from a new Japanese stellarator, the LHS, or Large Helical System. This three-quarter-billion dollar facility will be built at the new fusion institute at Toki near Nagoya and will be completed in 1995.

Our ability to exploit the capabilities of ATF and build on the promising initial results is being severely limited by the FY1989 budget. It will be compromised even more at the proposed level for FY1990. Total ATF funding in FY1988 was \$18.2M. This is close to the roughly \$19M per year which DOE presented to Congress about a year ago in its five-year magnetic fusion funding plan which included building the Compact Ignition Tokamak. Nineteen million, while not sufficient to allow full exploitation of ATF's long-term potential for steady-state operation, would allow timely and efficient use

of the over \$100M investment in the device and its support facilities.

The initial FY1989 funding for ATF operations was almost \$18M (17.695), but was subsequently reduced to just under \$15M (14.895). Operations support had to be substantially decreased, and as a result experimental operating time will fall by about 50%. It also required us to defer and/or reduce the scope of the ion cyclotron heating, divertor, and neutral beam improvement tasks. Fabrication and installation of diagnostic instrumentation is proceeding, but utilization of these important tools will be slowed because of staff reductions and restricted operations.

The proposed budget of \$13.75M for FY1990 would stop almost all diagnostic fabrication and installation and would require further substantial reductions in staffing. Timely and effective contributions to the confinement initiative will be extraordinarily difficult, if not impossible. While we were able to accommodate the FY1989 staff reductions by attrition and by placements within the Oak Ridge complex, our ability to respond to additional cuts is not so clear.

We hope that additional funding can be provided so that ATF can continue the productive and valuable research program which is just now starting.

Implicit in our assumption that increased understanding will lead to improved plasma performance is the availability of the magnetic, heating, fueling, and heat removal components to produce the conditions the plasma physicists say are needed for the better mousetrap. Historically, major parameter advances have resulted as much or more from new technology as from physics insight. High fields on Alcator-C, neutral beams and carbon limiters on PLT, pellet fueling on Alcator-C and D-III, and now computer-based fabrication techniques on ATF are major examples of this interaction.

This technology base is the "infrastructure" of fusion and must not be neglected even as the community turns more towards physics. We at Oak Ridge are proud of the role we have had in developing the technology that is now in use on devices throughout the world. Recent experiments on the Joint European Torus (JET) have utilized a pellet injector which was built at Oak Ridge to make major advances in the parameters of radio-frequency-heated plasmas. Scientists from Princeton and

ORNL have participated in these experiments in England under the auspices of a U.S./Euratom collaboration on plasma fueling.

Oak Ridge is also continuing collaborations in pellet fuelling and rf heating with a large number of labs in the U.S. and abroad.

A milestone in international collaboration was reached this year with the formal conclusion of the IEA's Large Coil Task. The participants in that effort have been asked by the IEA to look for ways to build on the experience and facilities of that program to further develop superconducting magnets for fusion applications. This is difficult in light of an almost 50% reduction for funding in magnetism work at ORNL during FY1989 and the proposed zeroing of the effort in FY1990.

While discussing infrastructure, I believe it is important to note the contributions that fusion technology makes to U.S. industrial development. High-frequency microwave technology developed at Oak Ridge is now being used in the development of improved ceramics and in the development of improved plasma sources for semiconductor processing. Similarly, the understanding gained from the development of materials to withstand the fusion environment has provided the basis for improved high-temperature steels which will lead to more efficient electric power production. Active collaboration with industry is an essential part of these tasks, and I would like to compliment the subcommittee chairman on her legislative efforts to facilitate those interactions.

As I indicated earlier, progress is being made on the development of materials for fusion reactors. Previous research at ORNL showed that modifications to conventional stainless steels produced materials that were acceptable for use in first-generation fusion reactors. More recently, new alloys have been developed that would result in a thousand-fold reduction in the radioactivity of the waste from a fusion reactor. Deep burial of this waste is not required (at least under present regulations).

In addition to these structural alloys, so called "special-purpose" materials such as carbon-carbon composites, copper alloys, and ceramics are now being evaluated. These materials have important applications in next-generation engineering and

physics test facilities as well as in demonstration and power-producing fusion reactors.

As with any energy technology, fusion's ability to pass the test of economic and commercial viability will depend on materials. In fusion, the last test of public acceptance may well also depend on materials development.

ORNL's ability to both meet the materials needs of next-generation engineering test facilities, and to carry out the long-range development needed for economical and environmentally attractive fusion reactors has been compromised by budget reductions in FY1989. Proposed reductions in FY1990 will further distort this balance. Similarly, FY1989 reductions in support for rf technology and in commitments to support present facilities are reducing our ability to do long-term development. We hope that funds can be added to these areas to maintain a balance between short-term and long-term development.

The next-generation devices I discussed earlier are represented by the CTF and ITER, led by Princeton and Livermore respectively. ORNL is actively engaged in the physics and technology development of these projects and, with the Fusion Engineering Design Center, is contributing to the engineering. In addition, we are collaborating with UCLA on the ARIES advanced reactor study. It is through these studies that both the potential and the problems of fusion are best seen.

The price of this progress is a better understanding of the problems that remain to be solved. The broad range of physics, technology, engineering, and ultimately economic and environmental issues which we face are certainly formidable. However we must remember that fusion is one of the very few energy options available to us in the long run. Our response to the understanding of a greater technical challenge must be the maintenance of a perspective on how far we have come and a rededicated attack on the problems that remain.

Mrs. LLOYD. Thank you very much, Lee.
Dr. Furth, please proceed.

STATEMENT OF DR. HAROLD P. FURTH, DIRECTOR, PRINCETON
PLASMA PHYSICS LABORATORY, PRINCETON, NY

Dr. FURTH. Madam Chairman and Members of the Committee, thank you very much for inviting me. Since time is short I'm going to concentrate on projects of particular interest to my laboratory; namely, TFTR, CIT, and PBX-M, but I will not be able to resist leaving some general philosophical remarks about the program, and particularly the topics of program balance and program predictability which I know that you're interested in.

So let me start with TFTR and say something on the subject of program balance, which is quite well illustrated by TFTR. We would like to achieve progress in science. We would like to increase performance and push parameters toward break-even and later to ignition. And, as the gentleman on my left said, we would like to make a better mousetrap because it is our ability to point to a really good mousetrap which in the end justifies the funding for this whole enterprise. So those three things have to progress together, and in TFTR, happily, they have.

Some good experimentalists have pushed the plasma temperature to 300 million degrees, the full temperature you will ever want in a reactor. When the theorists asked, "How on earth did this happen?" more experiments were done and a great deal was learned about fundamental confinement physics, which is most instructive. When that was learned, the people had a clue how to build a better mousetrap, and the tokamak mousetrap at that point was improved.

Looking to the future, confinement physics and better mousetrap technology working together are going to tell us how to further increase performance on TFTR as we move towards the break-even goal.

Where are we relative to break-even? At the moment all fusion experiments in the world are being done in deuterium plasmas so that one achieves a certain level of confinement in deuterium and then one extrapolates to what that would do in deuterium-tritium. There are various ways to do that extrapolation but by the most realistic way we now have a factor of 2 to go.

To put that into proportion, when the TFTR project started in the mid-70s there was a factor of 10,000 to go. What has happened meanwhile is that this measure of energy multiplication, the Q value, has doubled 13 times, and what we propose to do is to double it once more.

Now we think we can do this, but at the same time we cannot prove it. Because our whole enterprise is to do new physics. And, if we could start by predicting exactly what would happen, then this committee or the Department would say, "Obviously, this experiment is not needed. You already know what's going to happen." And so we have to be a little careful, and we do not claim that we can predict it. For instance, no way could we have predicted 300 million degrees temperature in TFTR. Had we even claimed it 3 years ago people would have said, "You're dreaming." These things

just happen, and to get them to happen you have to aim and aim high.

Last year, if you will look at my testimony, I told you we were hoping within the year to come within a factor of 2 of break-even, and that is exactly what we have done. So sometimes one has premonitions but one cannot promise or swear or be certain. That's the way physics goes.

In terms of international competition, we were pleased to note at the last international meeting in October that we are a factor of 2 ahead of the nearest competition, which is the JET device in England, and that is so even though the TFTR budget is substantially smaller than that of JET of JT-60, the lead machine in Japan.

Turning now to the TFTR budget, it was cut by \$6 million in 1989. It will be cut by \$13 million in 1990. That does not impact our ability to achieve break-even conditions in deuterium, and we plan to go toward that goal full speed. It does not permanently in any way damage our ability to do deuterium-tritium experiments, but for the moment the systems preparations for that have been suspended. And so, while there is a great benefit, as Dr. Hunter has mentioned to you, in our ability to use this marvelous facility unique in its high temperature, to do front-line physics and to develop the tokamak mousetrap, there is also a price being paid; namely, the deuterium-tritium burning schedule has been set back.

And that brings me to my second philosophical topic, namely, predictability. The Magnetic Fusion Energy Engineering Act of 1980, which I have recently reread, envisaged that there should be a real engineering test reactor running by 1990, and it says "or sooner." And it also envisaged a doubling of the magnetic fusion budget in real dollars. Now what has actually happened was not a doubling but a halving, and so let me make the following general remarks.

If one is going to launch a serious deuterium-tritium burning program aiming step by difficult step toward a reactor, what one needs is predictability. In fact, what you need is a predictably rising budget, at least moderately rising, so that one step can feed into the next and we're not left with the danger of having completed a step and has nothing to follow on. If there is lack of predictability, you get wasted investments. If the budget is predictable, but predictably falling or predictably stagnant, then trying to proceed with a major D-T burning project is bound to upset the balance of the program. And so the bottom line is we should enter into a serious D-T burning program. We can do it technically, but what it needs is predictably (at least moderately) rising budgets.

Let me then turn to CIT. We have a national design team at work who in the past year have made very excellent progress. The question, Mrs. Lloyd, that you asked me last year was a very pertinent question; namely, will it work? And so let me immediately focus on that.

If we were building a high-energy accelerator, we would certainly have to swear that it's got to work and produce the beam. And similarly, as we build the CIT we must certainly swear that it will produce the magnetic fields and the currents and the heating that has been promised, otherwise our name is mud. That is perfectly clear.

But just as in when you build a high-energy accelerator you don't have to promise how many elementary particles you will find or even that you will find elementary particles, we cannot make any promises about exactly what the plasma will do. That is, in fact, the point of the exercise, to find out what it will do.

And so turning to this issue that has been touched on, can we predict ignition, let me tell you how in our view that stands. We are very good at predicting the worst case, the so-called Low Mode. With rather small scatter, we can project that to the CIT regimes. And it is clear that, depending on how powerful a CIT machine we build, we will need enhancements in confinement by a factor of 1 to 2 in order to reach ignition. Now such factors are reached every day in Dr. Overskei's machine, in our machine—all over people reach such factors. But whether those will project precisely to CIT is something we simply, in our state of knowledge cannot now predict.

And so we have evolved a strategy which in response to your question I presented last year, which is, since we cannot be sure let us start by building a machine which has got the full machine potential for very strong fields and for very high probability of ignition with very little needed margin of enhancement, but let us not initially spend all of the money for the power supplies. Let us rather initially see if we can make it work through ingenuity with a factor of 2 enhancement in confinement. Because if we can, firstly, we save all that extra money; and, secondly, what we have done is far more useful for posterity because then they can use that technique and they can build a cheaper ITER and they can build cheaper reactors, so everyone will be way ahead.

So that thought led to the 2-phase strategy which I explained to you last year. We begin with something that requires a large enhancement in confinement, factor of 2, and we try to produce it, but we do have a backup. If we add additional power supplies, and here we're speaking of about \$100 million worth roughly of power supplies, we can then raise the magnetic field and operate CIT at a level where very little enhancement is needed and people would tend to grant it would be very likely to ignite.

One other factor on that is in the initial embodiment in the PACE cost there is only a moderate amount of heating power. As we mentioned last time, it would be very desirable to provide some extra tens of megawatts of advanced electron-cyclotron heating. That would greatly up the probability right there. That would help.

Now we could have followed a third alternate, and initially we did. That we don't aim for 2 phases, we aim at just 1 machine. And that is, of course, something you can do, but that 1 machine then will have more margin within your first phase but less margin within your ultimate phase, and so in some sense it is a bigger risk. And that is why we decided to split it into 2 phases.

What's happening on the CIT engineering effort? A lot of good work has been done, not just in drawing blueprints, but in really prototyping parts and testing them and the base design has been pretty much verified now as being workable. Even better news, during the past year the team has invented a clever new design which is within about 2 weeks from being accredited as the winner—and we hope it is the winner—which would give consider-

ably bigger parameters, considerably bigger performance without raising the project cost a nickel. It would raise both the initial capability and the ultimate capability. And so, if that holds up, and I think it will, it will be a great benefit.

On the budget side of CIT, we had hoped until recently to have \$31 million of construction money in '90, but the President's budget has \$5.5 million. The impact of that is to slow down the construction by about half a year, so experiments will really now start in January of '97 instead of mid-96. And on that score, if at this point one could still raise that \$5.5 million to something more like \$20 million, that loss of time could largely be recovered.

Mrs. Lloyd, I was pleased to hear you mention something which I think is very true and very important. That the interest of the international community in chipping in and helping us is very closely correlated with what they sense our seriousness is. And when they say serious, what they look at is things like what PACE money are we laying out. So that, there's a big multiplier here. If we could convey a greater sense of seriousness, whatever additional funding is put in I think could be multiplied through foreign contributions—and the converse is also true.

Let me say, finally, a word or two about PBX-M, which is our machine for looking at advanced tokamak concepts, and that again has to do with the subject of balance. PBX-M is designed to do new physics. It is designed to make a much better mousetrap. In fact, PBX-M is aimed at making the best possible mousetrap for a tokamak, which has already been alluded to by Dr. Berry; namely, the second stability regime. PBX-M was designed specifically for that purpose and is generally regarded as having the best crack at proving the existence of this regime in tokamak.

What that has to do with is raising the so-called beta value, the—raising the plasma pressure relative to the magnetic field strength on which over the years there has been a friendly and I think very profitable competition going on. The DIII device at GA for a long time held the world record. Then in '86 PBX came in and got it by a nose. In '87, DIII-D, the more advanced version, came back and got it back by a nose. And all of that has caused, of course, the beta value to rise and keep rising, and so I'm pleased to tell you and Dr. Overskei that just last week PBX-M, the improved version of PBX now, went sailing past the old PBX mark and as he looks back from the boat there's PBX-M closing in fast.

Now there's just one catch to that, and that is again something to do with budgets. The budget for PBX-M has been \$12 million, and the budget for fiscal '90 is very easy to remember because it's zero starting October 1st. And why is that? It is because in this famous 5-year plan that was mentioned in '90 and '91 there was supposed to be some great bulge in TFTR and in CIT; and PBX, while by no means being stopped, was supposed to be put on ice in order to compensate for this enormous bulge. And coming back to the topic of predictability, that prediction was not good. There is no enormous bulge. But at the moment we still have zero budget for PBX, which from a technical point of view I think no one would be prepared to defend as a reasonable decision.

Mr. Morrison, I was pleased to hear, that he and others would like to see investments utilized properly, and in this case it

is not just a matter of utilizing an investment, it is a matter of utilizing a very successful investment which otherwise would be halted on the brink of even greater success. And I very much hope that this committee will not let that happen.

Well, finally, since we are on gloomy topics, let me make a comment on the institutional situation of the Princeton Plasma Physics Laboratory. At the beginning of fiscal year '89, the present fiscal year, we had \$109 million. At present, halfway through we have \$103 million. In fiscal '90, we're looking at \$97 million. (From all of those one should subtract \$7 million for CIT monies actually spent at other laboratories.) As a result we have recently laid off 160 subcontractors, we have reduced the laboratory from 1,000 to 900 positions, we have laid off 60 employees. It has been a dislocating experience, and there seems to be sort of a mismatch between the great aspirations I've been telling you about and these disturbing circumstances which make it difficult to meet the aspirations.

But let me nonetheless close on a positive note, to repeat what those aspirations are:

In TFTR we hope and expect that we will reach break-even conditions in deuterium during '89 and '90 in spite of our problems, and that we will then go on and burn deuterium-tritium and utilize, Mr. Morrison, the very substantial investment in deuterium-tritium burning capability that has already been made and make a great success of it.

In PBX we're aiming at making the best of all possible tokamaks, and we very much hope that we will be given a chance to do that.

In CIT we and our colleagues from around the Nation are sowing the seeds for the burning plasma program of the future which will lead to real fusion reactors, and I hope and know that you vigorously support that idea.

Thank you very much.

[The prepared statement of Dr. Furth follows:]

Statement of Harold P. Furth
Director, Princeton Plasma Physics Laboratory
before the
Subcommittee on Energy Research and Development
House Committee on Science, Space, and Technology
21 February 1989

Madame Chairman, Members of the Committee:

The development of an environmentally benign long-term energy source is one of the most important scientific and technological challenges of the present era. World magnetic fusion research has been progressing steadily to meet that challenge.

The mission of the Princeton Plasma Physics Laboratory is to develop a practical fusion-reactor core, by perfecting the tokamak approach to plasma confinement. Our program consists of two parallel, interrelated efforts: improving tokamak plasma confinement and initiating the exploration of burning deuterium-tritium (D-T) plasmas. Achievement of the burning-plasma objectives will depend on the success of the plasma-confinement effort and on the implementation of a well-supported, predictable, long-term plan for the U.S. magnetic fusion research program.

Last year, I discussed with you the technical progress and program strategy in the burning-plasma sector of the U.S. magnetic fusion effort. I described our plans for advancing toward a break-even experiment in the Tokamak Fusion Test Reactor (TFTR), and toward construction of a next-step deuterium-tritium plasma device, the Compact Ignition Tokamak (CIT), which will explore the physics of fully reactor-like burning plasmas.

A number of major technical objectives that I mentioned in my testimony to you last year have meanwhile been achieved: The TFTR experiments in deuterium plasmas have actually advanced to within a factor of two of break-even conditions. The CIT design effort has succeeded in improving the machine parameters without exceeding the previous cost constraint. In the area of improving the understanding and quality of tokamak plasma confinement, the TFTR has made major new discoveries, and the PBX-M device has started a series of experiments that have excellent prospects of achieving the ideal maximum-plasma-pressure regime of the tokamak.

As you know, during the past year there has been a redirection of DOE strategy in magnetic fusion research, with increased emphasis placed on plasma confinement physics, and reduced emphasis on the near-term burning-plasma program. My testimony today will address the further contributions that the Princeton Plasma Physics Laboratory can make to the development of high-temperature plasma-confinement physics using PBX-M and TFTR. I will also discuss the serious consequences of the change in program emphasis for the D-T phase of TFTR, for the CIT project, and for the institutional health of the Laboratory.

The Tokamak Fusion Test Reactor (TFTR)

In the summer of 1986, the TFTR experiments discovered a new tokamak plasma regime, in which the fusion reaction rate greatly increased and the ion temperature surpassed 200 million degrees. Subsequent TFTR experiments have extended the parameter range of this "supershot" regime, reaching fully reactor-like temperatures above 300 million degrees and coming within a factor of two of the break-even point -- i.e., reaching conditions in

deuterium plasmas that would extrapolate to an energy multiplication factor of $Q = 0.5$ in a deuterium-tritium plasma. These results were reported in October, 1988 at the biennial IAEA meeting on controlled fusion, and were recognized as setting new standards for tokamak performance, exceeding the parameters of the somewhat larger and substantially better-funded lead tokamak experiments of Europe and Japan.

In reaching the $Q = 0.5$ mark, TFTR satisfied an approximate criterion for significant D-T plasma-physics research capability that was suggested in 1987 by Panel XVII of the Magnetic Fusion Advisory Committee. In the context of the historical goals of the TFTR project, a Q -value of 0.5 falls well within the target range of $Q = 0.1 - 1$ that was proposed at the start of construction in 1976 and exceeds the Q -values that had been obtained at that time by a factor of ten thousand.

On the budgetary side of the TFTR break-even effort, there has been a significant down-turn since last year. In response to DOE instructions, the program of D-T preparations was discontinued in November 1988, and the overall TFTR budget was cut, relative to earlier DOE plans, by \$6.0M in FY'89 and by \$13.0M in FY'90. The present DOE plan calls for postponement of the TFTR burning plasma studies by about three years, to the end of 1993.

Even in these adverse circumstances, we will be able to maintain the prior schedule of the TFTR project for experimental progress towards demonstration of the break-even regime in deuterium plasmas (i.e., during FY'89-'90). The exploration of the physics of supershot plasmas is also yielding important new physics insights into means for optimizing tokamak performance in general -- particularly in maximizing fusion energy multiplication factors and in creating a basis for steady-state tokamak

reactor operation. During recent years, TFTR has ~~not~~ fully exploited its physics potential, in order to maximize the resources made available for D-T preparation. Modest additions of hardware can significantly extend the capability of TFTR to do advanced deuterium plasma experiments. The new physics results will be important for CIT, for the International Thermonuclear Experimental Reactor (ITER), and for the projection of an economical tokamak power reactor.

The Princeton Beta Experiment (PBX-M)

A particularly attractive reactor-plasma solution would be obtained by raising the moderate-pressure TFTR supershot plasma into the so-called "second stability regime," a high-pressure plasma state that is predicted by theory to provide stable confinement. The plasma geometry of the PBX device is designed specifically to maximize the probability of entering this second stability regime. During 1985-86, PBX set world records for high-pressure plasma confinement in the "first stability regime," and was then refurbished with a number of promising new features. An initial goal of the PBX-M experiment is the demonstration of the existence of the second stability regime. We could then hope to develop techniques for entering this superior regime in tokamaks of less specialized geometry, such as TFTR, CIT, and ITER.

The PBX-M experiments are currently underway, and the initial results are promising. The critical problem for this key element of tokamak confinement-physics research is that the near-term budget outlook is particularly grim: To compensate for the upsurge of CIT construction budgets that had been expected for FY'90 and FY'91, the PBX-M annual operating budget of \$12M was zeroed for these two years in the DOE program

plan of February 1988. Meanwhile, the increase in CIT funding has not materialized (see below), but the PBX-M experimental program is still scheduled to be interrupted in October.

The Compact Ignition Tokamak (CIT)

Last spring, the Committee asked me to comment specifically on the capability of CIT to achieve the desired burning-plasma state. At that time, the national CIT project team and DOE had just decided on a two-phase strategy: to build a somewhat larger CIT machine, with a greater ultimate capability, while maintaining the initial CIT project cost of \$586M (as-spent dollars) by an additional expenditure of approximately \$100M (FY'89 dollars) for incremental power supplies, which would be needed to operate the machine at its full design parameters.

As I mentioned in my testimony to you last year, the idea was to emphasize the application of special plasma-handling techniques to achieve ignition in CIT at moderate values of magnetic field strength and plasma current -- which would constitute the most valuable possible contribution toward low-cost burning-plasma operation in future tokamak reactors. At the same time, the latent capability of the CIT device to operate at substantially higher machine parameters would raise the margin of confidence in the achievement of ignition.

Meanwhile, the CIT project has made significant progress in a number of areas. The CIT national team has successfully tested the most critical components of its magnet coil systems, and has initiated detailed design of the vacuum vessel. We are currently evaluating an innovative design for the magnet coil system which has the potential to produce substantial improvements in the initial and ultimate machine parameters. We now expect

to operate CIT initially at a magnetic field strength of 8 Tesla, with a plasma current of 9 Megamperes. The ultimate parameters may be as high as 11 Tesla and 14 Megamperes.

On the budgetary side, the progress of the CIT project has been difficult, in spite of the strong and consistent support of this Committee. For FY'90, DOE initially proposed a PACE funding level of \$31M which has been reduced to \$5.5M in the President's budget request thereby extending the construction schedule for CIT.

Concluding Remarks

During the past year, technical progress at the Laboratory has been very strong. TFTR is making major scientific discoveries and is coming increasingly close to its break-even goal. PBX-M is developing a superior tokamak configuration that should be able to provide insight into a whole new branch of tokamak confinement physics and would convincingly demonstrate the existence of an attractive fusion power-reactor solution. The CIT project has developed design improvements within its existing budgetary envelope that promise to yield substantial enhancements in performance.

Looking ahead to FY'90, the Laboratory faces a number of serious funding problems. The TFTR D-T preparations are being postponed, the PBX-M experiment is to be shut down, and CIT construction funding is reduced to a minimal level. The total PPPL budget has been cut by \$12M, from the initial FY'89 level of \$109M to the proposed FY'90 level of \$97M. As a result, about 160 subcontractor jobs have been eliminated, and the Laboratory staff has had to be reduced from about 1000 at the outset of FY'89 to 900 at the outset of FY'90, including a lay-off of about 60 employees.

At the present time, the Princeton Plasma Physics Laboratory is still able to maintain the position of U.S. fusion research at the cutting edge of the world effort. We seek the Committee's support for our important plasma confinement experiments, and for the implementation of a stable, long-term experimental program in the burning-plasma sector of U.S. magnetic fusion research. With the Committee's help and guidance, we can complete the scientific basis for a convincingly attractive fusion power reactor during the coming years, and continue strong and steady progress toward the realization of fusion power.

Mrs. LLOYD. Thank you very much, Dr. Furth.
Dr. Linford?

**STATEMENT OF DR. RULON K. LINFORD, PROGRAM DIRECTOR
FOR MAGNETIC FUSION ENERGY, LOS ALAMOS NATIONAL LAB-
ORATORY, LOS ALAMOS, NM**

Dr. LINFORD. Madam Chairman and Members of the Subcommittee, I appreciate this opportunity once again to testify. In Chairman Lloyd's letter she asked that I provide information on the Confinement Physics Research Facility, or CPRF, that you've heard about a little bit earlier today that is under construction at Los Alamos. In particular, she asked that I comment on budget cost increases, descoping of the project, and the impact of that descoping on the expected results of this facility. These are important questions and warranted a more detailed response than I'll have time to present orally today, so I've added some greater detail to my written testimony, and would ask that it be accepted into the record.

Mrs. LLOYD. Without objection.

Dr. LINFORD. The CPRF is a facility which is being constructed to initially test the next step in the Reversed Field Pinch, or RFP, program. That experiment is called ZTH. The facility, however, has a longer term purpose, and that is, to be able to test alternate concepts in the future, particularly those that have the potential for high-power density.

In my testimony last year I talked about the cost increase for the CPRF and the resulting descoping of the plasma current capability from 4 megamps to 2 megamps. I also mentioned that we anticipated a new data sheet for the project would be submitted to Congress. Unfortunately, the re-examining of the scope of the project took longer than anticipated, but I'm pleased that Dr. Hunter was able to send the data sheet to Congress last month. This Schedule 44 data sheet is for \$75.6 million, of which about two-thirds is for the facility and about one-third for the ZTH experiment. The project completion date is expected to be the second quarter of fiscal year '93, and the plasma current will be 1.7 megamps. That 1.7 megamps could be increased to 2 megamps by the addition of a single ohmic heating supply. But the increase from 2 megamps back up to the original 4 megamps would require the addition of some additional structure to support the coils in the ZTH experiment as well as increasing all of the power systems to accommodate that higher plasma current.

The increase in projected cost of the full 4 megamp system occurred because the contingency was inadequate for the early stage of the design. The original estimate increased from \$72.5 million over a 9-month period to \$98.9 million. This increase occurred as the preliminary design matured and as we instituted a management tracking program which allows us to do an earned value analysis each month. We notified the Office of Fusion Energy in October of 1987 about that cost increase, and they called a committee of senior project people in the fusion program to examine the budget estimates.

The cost review committee checked the estimates leading to both the \$72.5 million and \$98.9 million figures and found them to be both reasonable, but pointed out that the contingency in the original figure was inadequate. OFE, as a result, requested that we descope the project sufficiently to fit within the original \$72.5 million, and we did that by descoping it from the 4 megamps to 2 megamps, as I have previously indicated. However, they also indicated that because of the decrease in the overall national budgets that the funding profile for the CPRF that had been originally planned could not be continued, and therefore a combination of that reduced funding profile as well as the descoping resulted in the stretch-out of the construction of the project to then be completed in December of '92. This stretch-out resulted in a cost increase to \$75.6 million, which is the figure that you now see on the Schedule 44. A second cost review committee was convened to examine this estimate and it was validated.

The further descoping from 2 megamps to 1.7 megamps was necessitated by the requested reprogramming action this fiscal year which removed \$3 million from Los Alamos: \$2.5 million from the project and a half million dollars from the research operations at Los Alamos that have had to drop to only \$4.3 million. And it is difficult to maintain the scientific staff in a productive manner at that level but we are doing it. The result of this reprogramming was to stretch out the completion of the project until February of '93. We were asked to keep the total project cost at \$75.6 million and that required the descoping from 2 megamps to 1.7 megamps.

Since the descoping from 4 megamps to 2 megamps over a year ago the project has been on track. We have costed \$27.9 million compared to the amount that was budgeted for that work, which was just \$100,000 larger. The project is 39 percent complete; that is, the amount that has actually been costed. As Dr. Davies mentioned earlier, the commitments are approximately 50 percent of the project at this time.

To point out that we are on track, the most recent milestone was the completion of the foundation for the generator which was scheduled in December, and it was completed on time.

Now let me comment a little bit about the impact of this descoping. The next steps in the International Reversed Field Pinch program are the RFX, being constructed in Italy, and the ZTH device that I've been talking about, at Los Alamos. The reason for setting the current capability of these experiments at 2 megamps or greater was to access a new physics regime in the RFP to allow us to separate competing MHD, kinetic and atomic physics effects on the transport in an RFP. This allows us—would allow us to better assess the potential of the RFP including its capability to be ohmically heated to ignition.

A recent reactor study on the RFP, called TITAN, has been completed. It was led by UCLA, and the results support the potential of the RFP that I've talked to you about in previous testimonies. It has also motivated specific RFP research that is being carried out at the present time, and the results are very promising and we are very pleased with the scientific progress of the RFP from the international perspective.

When the CPRF and ZTH were assigned to Los Alamos at the end of 1986 the plan was to complete the construction of a 2 megamp phase within 1 year of the completion of the RFX in Italy and then 2 years later to complete the 4 megamp capability. Now the 1.7 megamp capability will be completed 3 years after RFX, and therefore in terms of our international interaction and cooperation it is less timely. The current is also lower. The 1.7 megamps is quite similar to the 2 megamps in terms of the physics regime that can be accessed, but our ability to separate the various physics effects that I mentioned earlier is more marginal. However, in my testimony last year I emphasized that the design of the ZTH was complementary with that of the RFX. It will yield unique results even at the 1.7 megamp level. For example, comparing results between the 2 experiments will allow us to assess the impact of equilibrium control and field errors on transport in the RFP. In addition, the ZTH allows the study of MHD activity with a thin shell and hot plasmas in the kilovolt regime. These studies are vital for assessing the RFP potential.

Cooperation between the United States, Japan and the European Community is beneficial to all parties. The formalization of an IEA trilateral agreement on the RFP is expected soon. A new RFP experiment has recently been completed at the University of Wisconsin called the MST, and it also fills an important role in the international program.

Considering the complementary design of the ZTH, RFP and MST, the upgradeability of the ZTH and the long-term utility of the CPRF as a facility, the CPRF/ZTH is still a cost effective investment. However, further stretch-outs or descoping would begin to seriously degrade the value of the ZTH experiment itself. It is important, therefore, to complete the construction at least as fast as presently scheduled. If the \$2.5 million were restored in fiscal year '90, the 2 megamp capability and schedule could also be restored.

Let me now make some comments with regard to the national program balance and needs. The requested reprogramming action seeks to improve understanding of transport, particularly in tokamaks. I support that increased emphasis as one of the issues needing attention in the fusion program. I'm interested in helping it succeed and am a member of the Transport Task Force Steering Committee. I, therefore, accept the fiscal year '89 reprogramming because of the importance to initiate this increased emphasis.

However, I disagree with the continuing shift of funds in the President's FY '90 budget from what I call concept improvement to the mainline tokamak. The concept improvement that I'm talking about, activity, includes both advanced tokamaks and alternate concepts, and the advanced tokamaks that were talked about by Harold Furth, the second stability regime tokamak like PBX, would be included.

Transport is not the only program issue that needs to be resolved to improve the development path for fusion that Dr. Hunter talked about today. The concept improvement area plays an important role in addressing these essential issues, but this activity comprises only 15 percent of the total budget before the reprogramming action. The mainline tokamak area has much larger resources.

Studying the transport in related field geometries such as the stellarator and the reversed field pinch can help unravel the physics of the tokamak transport as well as transport in those related geometries. Therefore, some of the concept improvement research could profitably be focused on a coordinated transport activity. But if this concept improvement program is to contribute, it must be viable. For these reasons balance, the balance between the concept improvement and mainline tokamak area should be restored.

Let me also mention, or make a few comments about the impact of decreasing fusion budgets. During the past several years of declining budgets difficult choices and sacrifices have been made to construct a few new experiments and upgrade others to maintain the vitality required for fusion to continue to make progress. Last year's President's budget request was for \$360 million and the 5-year plan that the chairman talked about earlier today projected a level of effort budget which would correspond to \$375 million in fiscal year '90. However, the fiscal year '90 budget that is now being proposed by the President contains only \$349 million. These budget reductions prevent efficient use of existing facilities both in the mainline tokamak program and in the concept improvement area, as you have heard today, as well as the efficient completion of those experiments presently under construction.

A \$375 million level is needed to efficiently use these facilities and to reap the benefits of that investment. Consistent with my testimony last year, an increment over \$375-million level of effort budget is needed to construct the CIT on a reasonable schedule. The study of burning plasmas is still the next appropriate major step for the fusion program. Excellent progress has been made in the fusion program in scientific understanding, in the discovery of new regimes of plasma behavior, and in advancing parameters towards the burning plasma regime, which also requires technological advances. Future success requires proper balance in the effort—in these efforts, and the potential for fusion to help resolve the growing demands for energy, the growing concern over environmental issues warrants the incremental funds needed to maintain that balance.

Thank you again for the opportunity to testify.

[The prepared statement of Mr. Linford follows:]

**TESTIMONY TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT OF THE
HOUSE SCIENCE, SPACE, AND TECHNOLOGY COMMITTEE
TUESDAY, FEBRUARY 21, 1989**

**RULON K. LINFORD
PROGRAM DIRECTOR FOR MAGNETIC FUSION ENERGY
LOS ALAMOS NATIONAL LABORATORY**

Madam Chairman and Members of the Subcommittee on Energy Research and Development:

I appreciate this opportunity to testify again regarding the Magnetic Fusion Energy program. Chairman Lloyd's letter to me requests information about the Los Alamos Confinement Physics Research Facility (CPRF), including information on cost increases in the project, descoping of the plasma current capability, and the impact of this descoping on the potential achievements of this facility, including its relevance in assessing the potential of the Reversed Field Pinch (RFP) concept to be ohmically heated to ignition. I have provided in this document a summary of some relevant historical and technical factors that are important in responding to these requests.

Before launching into this material, however, I can summarize the response with the following. The increase in projected costs arose because the original budgetary estimate did not contain adequate contingency to cover the normal cost increases that occur as the preliminary design matures. Since the initial descoping from 4 MA to 2 MA over a year ago, the project has been on track. The long-term utility of the CPRF facility and the unique design of the ZTH experiment continue to make them a cost-effective investment for the national fusion program; important, complementary contributions to the international RFP program are still expected even after the recent budget-driven descoping to 17 MA. Because of the stretched-out schedule, the results from ZTH will not be as timely relative to those of the 2-MA RFX device being constructed in Italy, and further descoping or delays in ZTH would seriously erode the value of the initial results from ZTH. However, the descoped design provides for efficient upgrading, which would enhance the prospects for even greater scientific understanding of the RFP potential and toroidal confinement in general.

Historical Background

The Reversed Field Pinch (RFP) is an alternate magnetic confinement concept with axisymmetric toroidal geometry similar to the tokamak. However, the internal structure of the magnetic fields differ from the tokamak and offer the potential of ignition by ohmic heating alone with magnetic fields at the coils that are substantially smaller than the tokamak. If further research with the RFP indicates that these potential advantages can be realized at the reactor level, then the result should be substantially smaller and simpler reactors not requiring auxiliary heating (neutral beams or radio frequency) or superconducting magnets.

These potential advantages coupled with the promising parameter achievements and important physics results from the ZT-40 experiment at Los Alamos motivated the recommendation, in the Magnetic Fusion Advisory Committee (MFAC) transmittal letter (October 19, 1982) of the Panel 2 report, to place high priority on the construction of the next-step device in the RFP program. MFAC noted that the RFP offered a qualitatively different product from the mainline tokamak, acknowledged the positive results from ZT-40. It recommended the "reference" program (ZT-40 Upgrade) as being appropriate for the constrained budget case considered by the panel. The ZT-40 Upgrade was proposed by Los Alamos as a 2-MA RFP with a total project cost (TPC) of \$44M that could be upgraded to a 4-MA capability at an additional cost of \$42.8M, which included the replacement of the magnet coils.

In January 1983, the President's budget for FY84 submitted to Congress contained a Schedule 47 for a new RFP device, called ZTH, which was described as a 0.6-MA machine (that could be upgraded to 2 MA) for a total project cost (TPC) of \$23.7M. Although it mentioned the need for a generator and power supplies, the capital equipment funds for those components were not included in that TPC. Los Alamos was then asked by the Department of Energy what could be done for \$29M, including the power systems. In September 1983, a letter from Dr. Harry Dreicer, Division Leader of the Los Alamos Controlled Thermonuclear Research Division, to Dr. David B. Nelson, then Director of the Applied Plasma Physics Division of the Department of Energy's Office of Fusion Energy, identified several options for the next RFP step including a \$29.7M ZTH option, with a 0.6-MA initial capability, ultimately upgradeable to 2 MA.

The FY85 President's budget submission to Congress did not contain a Schedule 47 for ZTH. The following July (1984), the main MFAC transmittal letter for the MFAC Panel 9 report acknowledged that progress in RFP research "has been outstanding" and recommended an increased budget for the RFP program and a next-step in the RFP program "in the 2-MA range or beyond." The MFAC further recommended that the "Los Alamos design options be examined to assure that the follow-on experiment to ZT-40 effectively complement the RFX experiment under construction at Padova" (Italy). In January and February of 1985 a ZTH technical review committee of fusion experts recommended that "the 4-MA large-bore (80 cm) experiment should be the principal goal of the program."

Early in FY85 discussions began between DOE, industry, and Los Alamos, on a jointly-funded new RFP or related OHTE experiment. In April 1985, 7 MA was agreed to as the project scope. In September 1985, Los Alamos was notified that it would be the site of the \$52.5M Confinement Physics Research Facility. This facility was to provide the test-bed for alternate concepts, and the first experiment was expected to be the joint effort between industry and Los Alamos on the next-step in the RFP program. In December 1985, DOE issued a formal solicitation of proposals for a cooperative agreement with industry. An estimated \$21M industrial contribution was needed for the experiment, while DOE would supply the \$52.5M TPC for the CPRF as shown in the FY87 Schedule 47. One year later, in December 1986, DOE announced that none of the industrial responses to their solicitation for participation were acceptable and assigned both the ZTH and CPRF activities to Los Alamos. The FY88 President's budget submitted to Congress contained a revised data sheet with a TPC of \$72.5M based on the \$52.5M for the CPRF and \$20M for the construction of a 4-MA ZTH device.

Increase in the Cost Estimate

Although the CPRF was assigned to Los Alamos in September 1985, prior to the beginning of 1987 Los Alamos had been instructed not to augment its MFE engineering staff in anticipation that industry might supply the needed engineers for both CPRF and the experiment as a major part of their contribution. Thus, the major preliminary design engineering was delayed until the staff could be built up during 1987. By October 1987, significant progress had been made on the preliminary design and associated cost estimates. Los Alamos advised OFE that on the basis of these more advanced cost estimates the TPC was now projected at \$98.9M, which included 16% contingency. OFE called for a review of the Los Alamos engineering and cost estimates, and also directed Los Alamos to descope the project to fit within \$72.5M. In December 1987, a peer review committee consisting of fusion program project engineers examined the original \$72.5M cost estimate and the subsequent increase to the \$98.9M level. They validated the \$98.9M cost estimate, and also found that the rough budgetary estimates that led to the \$72.5M figure were reasonable, but that the contingency contained in that figure (9%) was inadequate. The committee noted that the contingency should have been at least 25-30% to account for the increases in projected costs that normally occur (and did occur) as the engineering design matures.

Decoping, Budget Reductions, and Schedule Stretchouts

Los Alamos descope the project to fit within the \$72.5M by reducing the power supplies to a 2-MA capability and reducing the magnet support structure of ZTH to the same 2-MA level. However, due to budget pressures, DOE informed Los Alamos that the originally expected funding profile for the project could not be met and would have to be reduced for several years. The integrated shortfall through FY88 would be \$2.9M and additional short-falls were projected for out-year budgets up through FY91. This stretched-out funding profile coupled with the descope from 4 MA to 2 MA resulted in the projected completion of construction to be delayed from October 1990 (for the 2-MA phase of the 4-MA device) to December 1992 (for the 2-MA descope design). This stretched-out schedule again increased the estimated cost of the project due to escalation and less efficient use of manpower resulting in the TPC increasing from \$72.5M to \$75.6M. The DOE called for another review panel of experts to examine this new cost and schedule for a 2-MA system. Before the panel convened, the FY89 President's budget was submitted to Congress without a Schedule 47 for this project. The budget indicated that the project scope was being re-examined and the TPC targeted at \$52.5M. The DOE, therefore, asked the panel to examine what type of operating experiment and facility could be constructed for the \$52.5M in addition to requesting the panel to examine the \$75.6M case. The panel met in February 1988 and validated the \$75.6M cost estimate and schedule; they also concluded that a programmatically-relevant operating experiment could not be constructed for the \$52.5M, considering the portion that had already been spent.

A less detailed description of both the increased cost estimate and resulting descope of the project to 2 MA was given in my testimony last year (see attached copy, page 3).

Reprogramming and Associated Decoping and Stretchout

In October 1988, DOE informed us that \$3M would be removed from the Los Alamos program as part of a national reprogramming action. A \$0.5M portion of this cut was taken from the total research operations for both the Reversed Field Pinch and the Field Reversed Configuration programs at Los Alamos. This total research budget now is less than \$4.3M. The other \$2.5M was removed from the CPRF/ZTH project that forced the project completion date to be further delayed to February 1993 with an

associated increase in the total estimated cost. However, the DOE asked us to reduce the scope of the project rather than increasing the TPC above \$75.6M. This was accomplished by removing one of the four power supply modules for the ohmic heating system, thus dropping the initial current capability from 2 MA down to 1.7 MA. This choice would allow the least costly and least disruptive restoration to the 2-MA level if additional funds could be obtained at a later time.

New Schedule 44 Data Sheet for the CPRF

The re-examination of the scope of the CPRF, which prevented the submission of a data sheet with the FY89 President's budget request, was finally resolved when Dr. Robert O. Hunter, Jr. (Director of the DOE Office of Energy Research) sent letters to Chairman Roe and other members of Congress with the FY89 data sheet. Both the letter and the data sheet are attached. The FY90 President's budget request contains a Schedule 44 data sheet that is consistent with the FY89 data sheet. We are pleased that an agreement has now been reached on the scope, cost, and schedule of the project.

Project Status and Tracking

Since the descoping of the project over a year ago to 2 MA, the actual costs have tracked the budgeted costs; as of the end of December 1988, \$27.9M have been costed, as compared to the budgeted cost for work performed of \$28.0M. The project is 39% complete.

As I indicated last year, a new generator has been procured from TVA and has been shipped to Los Alamos. Now the generator foundation has been completed, and the initial portion of the contract for installation of the generator has been let. The magnet coils are being fabricated, the toroidal liner and shell assembly has just had its final design review, and preparations for the request for bids are underway. The preliminary design review for power conditioning systems is complete, and all other systems are progressing in a reasonable fashion.

Capability of the CPRF/ZTH

Three questions need to be answered in assessing the impact of the descoping from 4 MA to 1.7 MA:

1. What is the impact of reducing the physics regime that can be accessed and the importance of the results that would still be anticipated?
2. How does it effect the ZTH role within the world RFP program?
3. Is the CPRF/ZTH still cost effective?

The answers to all three of these questions are closely related. A major reason for the recommendation of the MFAC over four years ago to construct an RFP with a 2-MA or larger capability was that the step between the achieved 0.5-MA operation and the proposed 2-MA was assessed to be about the minimum step required to access a significantly different physics regime for the RFP, and thereby maximize the potential physics gain for the dollar invested. Theoretical and computational predictions have been made on how various resistive-MHD and kinetic processes will affect transport as the current and associated temperature and density are increased in the plasma. It appears that approximately 2 MA or greater is needed to separate the effects of these processes. However, there is no sudden transition, and therefore, the difference

between 17 MA and 2 MA is not expected to make a dramatic difference in the physics regime that can be accessed, but the ability to separate and identify the various physics processes becomes more marginal.

Understanding which processes dominate the transport is important in projecting the behavior of the RFP toward the reactor regime. For example, the present experimental data are consistent with a theoretical model that indicates that an RFP could be ohmically heated to ignition with a current in the neighborhood of 10 MA. This theoretical model predicts better confinement than was assumed in the recent TITAN reactor study (led by UCLA) which helped validate the attractive reactor potential of the RFP. However, the present experimental data are also reasonably consistent with theoretical models that have much less favorable predictions than would be needed to achieve the TITAN performance. Thus, ZTH should play a vital role in clarifying the dominant transport effects required to assess the potential of the RFP, including the potential of the RFP to be ohmically heated to ignition. Clearly this role would be strengthened if ZTH were eventually upgraded to the 4-MA level.

One of the reasons that the ZTH technical review committee recommended a 4-MA machine four years ago was that construction was beginning on the 2-MA RFX device in Padova, Italy. Thus, the 4-MA capability would have allowed these same physics studies to be extended even further into the high-temperature regime. This 4-MA capability was responsive to the MFAC request to ensure that the U.S. device would complement rather than duplicate the RFX. The descoping to 2 MA, and then 17 MA, does limit ZTH initially to the same plasma parameter regime as RFX, but other important design differences between ZTH and RFX preserve their complementary roles as was explained in my testimony last year (see attached, page 3). Comparing the results of the two experiments will be very important in determining the effects of equilibrium control and field errors on transport. In addition, the behavior of global modes in a high-temperature regime can be studied with the thin-shell design of ZTH. Close collaborative ties with RFP groups in the U.S., in Japan, and the European Community, continue to benefit all groups, and these benefits are expected to continue through the operation of both RFX and ZTH. A trilateral IEA agreement that formalizes these collaborations is close to being implemented.

A new RFP experiment called MST has just been completed and brought on-line at the University of Wisconsin. This device is designed to study plasma edge and boundary effects on the RFP, and its ultimate current capability (between 0.5-1 MA) can provide important data for currents intermediate between present experiments and RFX and ZTH. In addition, it has a larger minor radius than other existing experiments, and therefore, the results should provide information on the effect of size on the confinement properties of the RFP. MST could also be used to study the impact of magnetic fluctuations on transport as they apply to the RFP and to the tokamak, since the machine could be easily upgraded so that it could explore the entire range of configurations between the RFP and the tokamak. This will continue to strengthen the role of the RFP in understanding toroidal confinement physics. These are a few examples showing that the ZTH, RFX, and MST will play complementary and important roles in the international fusion program.

As previously indicated, the CPRF was initiated as a facility to explore alternate concepts, particularly those with high-power-density capability. As such, the facility has a longer-term value than the ZTH experiment, and CPRF represents two-thirds of the cost of the project. The majority of the ZTH components are capable of the full 4-MA operation. Even though it will provide important and unique physics information at the 17-MA level, the full investment would not be utilized unless the full current and

pulse length capabilities were added. The major cost, however, of the ZTH has to do with its unique design with a thin-shell, low-field errors, and active equilibrium control; precisely those elements that make it unique, even at 17 MA, and allow it to contribute in a complementary way relative to other facilities around the world.

Considering the unique features of the ZTH and the long-term benefits that can be derived from the CPRF, the investment is cost effective even at 17 MA. However, any further descoping or stretching out of the schedule would seriously erode the value of the investment in ZTH. If the recent cut due to the reprogramming were replaced in FY90, restoration of the 2-MA capability and an earlier completion date would be possible. This would result in an increased margin in the ability to achieve the desired physics results, and would allow the data from ZTH to be acquired in a more timely way relative to the operation of RFX.

Overall US Fusion Program Balance and Direction

Chairman Lloyd's letter asked me to comment on the relationship between various national fusion program elements and to provide my views on the optimum path for fusion. My views on this subject have changed little since my testimony last year, and therefore, a copy of that testimony is attached for your reference. I will comment here on only those areas where I feel that circumstances have changed significantly.

Last year, in considering fusion program strategy, we were contemplating an FY89 President's budget request of \$360M and a flat level-of-effort budget had been projected by the administration for subsequent years that would correspond to \$375M for FY90. At that time, I indicated that the study of burning plasmas in the ignited state is the next major step that should be taken in the MFE program. I also supported the construction of CIT at the \$360M level, with the understanding that incremental funding in FY90 beyond the flat level-of-effort projection (\$375M) would be needed for a reasonable CIT construction schedule while maintaining the program vitality required to address other essential issues in parallel with the study of burning plasmas.

I still support those views; unfortunately, the FY89 budget is only \$350.4M, allowing only some preliminary R&D activities to proceed on CIT. Instead of having the incremental funds above the \$375M level needed for the construction of CIT in FY90, the President's request contains only \$349.2M. This reduced level-of-effort in the program, even without CIT, makes it difficult to efficiently use existing facilities such as ATF, DIII-D, and PBX, and results in delays and associated inefficiencies in construction programs such as LSX and CPRF.

Another change that has occurred during the past year is the recent reprogramming action focused on increasing the understanding of transport in tokamaks. If this increased understanding could lead to improved confinement and better optimization of future large systems such as ITER, the performance per cost for these devices could be improved. Because of the importance for future major systems, I agree with the finding of the MFAC Summer Study that understanding transport is an area that deserves increased emphasis, particularly if additional resources were available to the program. I accept the FY89 reprogramming action because of the importance of initiating this emphasis on transport studies. I disagree, however, with the continuing shift of funding in the FY90 budget from the Concept Improvement activities into the Mainline Tokamak program in order to carry out this transport study. There are several reasons for my position. First, transport is not the only issue that needs resolution in order to improve the development path toward an attractive fusion power

source. Research in the Concept Improvement area, which includes alternate concepts and advanced tokamaks, is carried out to help resolve these issues. Second, the study of the related magnetic field geometries represented by the Concept Improvement experiments can aid both in unraveling the complex physics processes which contribute to transport, as well as uncovering potentially improved magnetic confinement geometries. Third, considering the need for Concept Improvement, it is well worth at least the roughly 15% of program resources being devoted to it prior to the reprogramming action. Since most of the program resources are devoted to the Mainline Tokamak program, shifting the emphasis within that area alone toward transport understanding would maintain a more reasonable balance. Focusing some of the research within the Concept Improvement area on coordinated research between alternates, advanced tokamaks, and conventional tokamaks would also be beneficial to enhance the understanding of transport and potentially improving it. However, this requires a viable Concept Improvement program. For these reasons, I believe it is important to re-establish the original balance between Concept Improvement activities and the Mainline Tokamak program.

Difficult choices and sacrifices have been made during the recent years of declining budgets to make a significant investment in constructing new or upgrading old experiments. The flat level-of-effort budgets proposed in the President's budget request last year are needed to efficiently reap the rewards of this significant investment. These investments have been made both in the Concept Improvement area and in the Mainline Tokamak program. Consistent with my testimony last year, if the FY90 budget could be increased to the \$375M level, it would help the MFE program to capitalize on the present investment in experimental devices, and an additional increment would allow CIT to be constructed on a reasonable schedule.

The fusion program has made remarkable progress in scientific understanding, in the discovery of new physics principles and regimes of plasma behavior, as well as advancing parameters towards the burning plasma regime. The future success of fusion requires maintaining a proper balance in the effort devoted to these various areas of progress. I believe the potential for fusion to help resolve the growing energy demands and concerns warrants the modest incremental funding needed to maintain that balance.

ATTACHMENT

**STATEMENT SUBMITTED TO THE SUBCOMMITTEE ON ENERGY
RESEARCH AND DEVELOPMENT OF THE HOUSE
SCIENCE, SPACE, AND TECHNOLOGY COMMITTEE**

April 27, 1988
Rulon K. Linford
Program Director
Magnetic Fusion Energy
Los Alamos National Laboratory

Madam Chairman and members of the Subcommittee:

I appreciate the opportunity to express my views regarding the overall status of the Magnetic Fusion Energy (MFE) program. I will try to be responsive to the questions asked in the Chairman's letter to me dated March 30, 1988. In responding to these questions I have found it useful to refer to my testimony to your Subcommittee last year, and I have attached a copy of that testimony for easy reference.

National MFE Budget Projections and the CIT.

Last year I expressed my support for the Department of Energy's plan to construct a Compact Ignition Tokamak (CIT) but pointed out the need for increased budgets in future years in order to maintain the base program at the level of effort that existed at that time. Unfortunately, the national MFE budget dropped to \$335M in FY88, instead of achieving the \$345M level in the President's requested budget at that time, or the increased budget which was originally in the House Appropriations Bill. In addition, the President's requested budget of \$360M for FY89 is followed by a flat level-of-effort projections for the years out through FY93. These flat projections do not contain the incremental funds I believe are necessary to construct the CIT; the \$360M for FY89 is only a cost-of-living increase over the \$345M budget I was anticipating for FY88 during my testimony last year.

In light of these budgetary changes during this past year and the continuing constraints on the Federal budget, my position with regard to the budget and the construction of CIT can be summarized as follows:

- The study of burning plasma in the ignited state remains the next major step to be taken in the MFE program.
- I support the \$360M in the President's request as the minimum increase (over the present \$335M) required to continue with a stretched-out construction of the CIT. However, incremental funding is required in FY90 and beyond in order to construct the CIT on a reasonable schedule while maintaining program vitality.

Program Vitality and Balance, and International Cooperation.

Chairman Lloyd's letter asked me to comment on three specific topics: the use of DT in TFTR, the CIT, and the ITER. These three activities constitute the main elements in one of the four key technical issues called out in the Magnetic Fusion Energy Program Plan. This issue of burning plasmas was described in Dr. James Decker's testimony to this Subcommittee, along with the other three key technical issues: magnetic confinement systems, materials, and nuclear technology. As he pointed out, the "Program's goal is to establish the scientific and technological base for fusion energy early in the next century," and "work on each of (these) four key technical issues" is required in order to meet that goal. In my view, however, the budgetary constraints prevent us from pursuing these key issues in parallel with

sufficient strength to meet that goal in the early part of the next century, even though international collaboration is used to the maximum extent possible in order to achieve the goal in a timely and cost-effective way.

Equating progress in the fusion program solely with progress in the burning plasma area is a common and easily made mistake. Burning plasma studies involve the largest and most expensive facilities, and the goals of breakeven and ignition are essential, easily measured, and easily understood. The excitement that is generated by progress toward these goals is understandable and justified. Although the achievement of these goals is essential, it is by no means sufficient to ensure the success of fusion as an energy source. As pointed out in the recently completed OTA report, fusion must provide a superior product in the areas of economics, environment, and safety if it is to successfully compete in the energy market of the future. Progress in the other three key areas is essential to be able to assess whether fusion could provide that superior product.

In an effort to maintain an appropriate balance between the four key issues, within the projected flat budgets, the President's projected budget includes the stretched-out construction of CIT and a slower pace for the other main elements of the program. The slower pace makes it more difficult to contribute on a par in the international community, and unfortunately counters the past efforts to coordinate program plans to maximize the complementary output from the international community. The slower pace also leads to less efficient use of our national resources, both personnel and facilities. The result is that small increases in the national budget would have very high leverage in increasing the productivity and maintaining the vitality and progress that has earmarked the MFE program in the past.

International Thermonuclear Experimental Reactor (ITER) Design and R&D

The ITER process is producing an important focus for a variety of ongoing R&D activities in the national program, and it has the potential for leading toward even improved international cooperation and effectiveness in these activities. The process should point out needs and begin focusing resources on achieving the physics and technological improvements needed for the experimental step beyond the CIT.

DT Experiments in TFTR

I concur with the MFAC assessment (charge number 17) that important scientific information would accrue from using DT in TFTR if the expected improvements in the operation of TFTR are forthcoming during the next nine months. The increased heating power available on TFTR during that period is projected to yield parameters consistent with values of Q between 0.5 and 1 if DT were used. The subsequent operation of TFTR with DT would not only result in important scientific information regarding alpha-particle physics but would achieve one of the major milestones for magnetic fusion.

Major Fusion Activities at Los Alamos

During the past year we have successfully operated the Tritium Systems Test Assembly (TSTA) at its full design performance of 100 grams of tritium inventory and a kilogram per day processing rate through the complete tritium fuel loop. The extremely successful separation of impurity gases deliberately injected into the fuel loop, and the successful containment and control of the tritium under a variety of operating conditions are important achievements for the fusion program and for the future use of tritium in TFTR, CIT, the ITER design activity, and follow-on experiments. This work has been carried out in collaboration with the Japanese.

Progress has also been made in the construction of the Confinement Physics Research Facility (CPRF) at Los Alamos which will provide the opportunity of studying the next step in the Reversed Field Pinch (RFP), and other compact fusion concepts in the future. A surplus 1430-MVA generator was obtained from the Tennessee Valley Authority and was transported from Tennessee to Los Alamos. This cost-effective acquisition of this surplus equipment is vital to CPRF. The generator will be capable for supplying 600 MJ of energy to the experiment. In the future, the deliverable energy could be increased to 2 GJ with the addition of a flywheel.

Last fall as the project progressed into the early construction phase, we recognized that the budget level of \$725M was insufficient for completion of the full 4 MA CPRF and associated RFP experiment called ZTH. To prevent additional pressures on the MFE budget, the project has been descope to an initial operation of 2 MA. The total cost of the descope CPRF has been peer reviewed and a new data sheet should be submitted in the near future. If it is found to be appropriate, the facility could be efficiently upgraded to 4 MA after initial operation at 2 MA.

Although this descope CPRF is not as large a step as we had originally planned, it is still responsive to the international RFP program. The only other device comparable to the ZTH is the RFX device being constructed in Padova, Italy. The RFX will also have a 2 MA capability and is planned to be operational in late 1989. Although the ZTH experiment is not planned to become operational for three years after RFX (partially due to a delay mandated by the flat budget projections), ZTH contains important features which make it an appropriate and complementary facility in the international RFP program. It has been designed for an efficient upgrade capability to 4 MA and to provide ohmic heating power densities that are an order of magnitude greater than that of RFX. The design of the ZTH machine allows the addition of impurity control measures such as divertors. In addition, it is designed to have a plasma duration long compared to the conducting shell time, which will allow us to explore equilibrium control and stabilization regimes not accessible by the present design of RFX. However, the early operation of RFX will allow us to become experienced with RFPs in the higher current and higher temperature regimes made possible by these two new devices. These and other complementary features of the two machines form the basis of international agreements being developed and implemented at the present time.

As I described more fully in my testimony last year, these international studies on the RFP are important in extending the understanding of RFP physics and scaling laws into the kilovolt temperature regime. These studies should lead to improved understanding of MHD behavior of high-beta, toroidally symmetric systems for the benefit of the overall fusion program. In addition, if the scaling laws remain valid into the ignition regime, the RFP could lead to a reactor that would not require auxiliary heating or superconducting technologies, resulting in a simpler, smaller, and lower cost device.

The RFP program is just one example of both ongoing and proposed areas of research in the national program that could significantly improve the potential of fusion to contribute to the world energy supply of the future.



ATTACHMENT

Department of Energy
Washington, D.C. 20585

January 12, 1989

Honorable Tom Beville
Chairman, Subcommittee on Energy
and Water Development
Committee on Appropriations
House of Representatives
Washington, DC 20515

Dear Mr. Chairman:

In its FY 1988 Budget Request, the Department of Energy (DOE) submitted a data sheet for the Confinement Physics Research Facility (CPRF) at Los Alamos National Laboratory (LANL), including a reversed field pinch experiment with a 4 mega amps (MA) current capability, at a total project cost of \$72.5 million and a completion date of September 1992.

When the FY 1989 Budget Request was submitted to Congress, the Department was still in the process of reviewing the CPRF project and, therefore, did not include a data sheet with its request. The review of CPRF was necessitated by a projected significant increase in cost, to about \$98 million. The Department directed LANL to descope the project to maintain the original cost, to the extent possible, while preserving enough capability to conduct key scientific experiments.

LANL's rescoping of the project has resulted in a \$75.6 million total estimated cost, with 1.7MA capability and a completion date of second quarter fiscal year 1993. This current capability is sufficient, according to theory, to conduct the highest priority reversed field pinch experiments relating to confinement at higher currents. All the elements of the descoped project were reviewed by a panel of engineers, experts in designing and building fusion experiments, who validated the estimated costs. The estimate was also validated by the Department's Program/Construction Management Procedures and Operations Division. Performance milestones for the project have been established. Management of the project has been strengthened by the implementation of a formal

performance measurement system and the addition of experienced project managers, both at LANL and at DOE. The Department is confident that LANL can and will carry out this project successfully. I am, therefore, submitting the revised CPRF data sheet. I would be pleased to answer any questions or provide additional information.

Sincerely,

Robert O. Hunter, Jr.

Robert O. Hunter, Jr.
Director
Office of Energy Research

Enclosure

cc: Honorable John T. Myers
Ranking Minority Member
Subcommittee on Energy and
Water Development
Committee on Appropriations
House of Representatives
Washington, DC 20515

Identical letter sent to the following:

Honorable J. Bennett Johnston
Chairman, Subcommittee on Energy
and Water Development
Committee on Appropriations
United States Senate
Washington, DC 20510

cc: Honorable Mark O. Hatfield
Ranking Minority Member
Subcommittee on Energy
and Water Development
Committee on Appropriations
United States Senate
Washington, DC 20510

Honorable J. Bennett Johnston
Chairman, Committee on
Energy and Natural Resources
United States Senate
Washington, DC 20510

cc: Honorable James A. McClure
Ranking Minority Member
Committee on Energy and
Natural Resources
United States Senate
Washington, DC 20510

bcc: Ben Cooper

Honorable Robert A. Roe
Chairman, Committee on Science,
Space and Technology
House of Representatives
Washington, DC 20515

cc: Honorable Manuel Lujan, Jr.
Ranking Minority Member
Committee on Science, Space
and Technology
House of Representatives
Washington, DC 20515

bcc: Carolyn Greenfield

DEPARTMENT OF ENERGY
FY 1989 CONGRESSIONAL BUDGET SUBMISSION
CONSTRUCTION PROJECT DATA SHEET
ENERGY SUPPLY RESEARCH AND DEVELOPMENT - PLANT AND CAPITAL EQUIPMENT
MAGNETIC FUSION

(Tabular dollars in thousands. Narrative material in whole dollars.)

1. Title and location of project: Confinement physics research facility (CPRF) 2. Project No. 89-R-000
Los Alamos National Laboratory (LANL)
3. Date A-E work initiated: 1st Qtr. FY 1986
- 3a. Date physical construction starts: 2nd Qtr. FY 1987
4. Date construction ends: 2nd Qtr. FY 1993
5. Previous cost estimate: \$72,500
Date: January 1987
6. Current cost estimate: \$75,600
Date: October 1988

7. Financial Schedule:	Fiscal Year	Appropriation Obligations	Cost
	1986	7,711	3,599
	1987	11,850	7,692
	1988	11,293	11,174
	1989	10,730	14,577
	1990	13,300	17,242
	1991	10,660	9,650
	1992	8,564	9,224
	1993	1,492	2,442

8. Brief Physical Description of Project

The CPRF includes an existing test cell and some space in an adjacent building. In addition, CPRF includes power, vacuum, control and data acquisition systems. The reversed field pinch (RFP) device within CPRF, called ZT-N, includes a vacuum vessel, shell, coils and associated structure. Also included are buildings for the power system and necessary modifications in the test cell and adjacent building.

☒ This revised project has been determined to be more appropriately funded as a line item construction project. This project was previously appropriated as an operating expense funded project through FY 1989.

CONSTRUCTION PROJECT DATA SHEET

1. Title and Location of Project: Confinement physics research facility (CPRF) 2. Project No. 119-R-100
Los Alamos National Laboratory (LANL)

3. Purpose, Justification of Need for, and Scope of Project

The purpose of CPRF is to provide a magnetic fusion research facility that is capable of exploring, in a multikilovolt-temperature, collisionless regime, the physics properties of a toroidal confinement concept that has the theoretical potential, in future devices, of ohmic heating to ignition with low magnetic fields. These physics properties include particle and energy transport and MHD activity in a high-beta, high-shear, low-q regime, including relaxation phenomena and the associated potential for steady-state current drive.

The presently operating reversed field pinch devices have achieved outstanding experimental results which surpass design specifications for the devices. For example, in ZT-40M, experiment duration has reached .5 milliseconds exceeding the design goal of 2 milliseconds. The Magnetic Fusion Advisory Committee recommended proceeding with the physics exploration of the reversed field pinch concept. The importance of extending the plasma current capability to values in the megampere range in order to test energy confinement was emphasized.

The initial research device that is to be installed in CPRF, and is included herein as part of the CPRF, will be capable of a 1.7 MA plasma current operating level. Certain key features of the CPRF will be sized with an ultimate capability of 4 MA, thereby facilitating a cost effective upgrade to the 4 MA operating level in the future, if warranted.

10. Details of Cost Estimate

- a. Engineering and design.....
- b. Construction related costs.....
 - (1) Procurement/fabrication.....
 - (2) Assembly/installation.....
 - (3) Construction.....
 - (4) Test/other.....
 - (5) R&D Activities.....

	Item Cost	Total Cost
a. Engineering and design.....		\$14,200
b. Construction related costs.....		53,300
(1) Procurement/fabrication.....	\$27,400	
(2) Assembly/installation.....	6,700	
(3) Construction.....	6,300	
(4) Test/other.....	10,100	
(5) R&D Activities.....	2,800	
Subtotal.....		<u>67,500</u>
c. Contingency at about 12% of above costs.....		<u>8,100</u>
Total Estimated Cost (TEC).....		\$75,600

Escalation is included at 5% per annum.

CONSTRUCTION PROJECT DATA SHEET

1. Title and Location of Project: Confinement physics research Facility (CPRF) 2. Project No. 19-R-800
Los Alamos National Laboratory (LANL)

13. Narrative Explanation of Project Funding and Other Related Funding Requirements (Continued)

(2) Related CFP Funds - These funds provide for various facility related tasks including weather protecting buildings for the power systems and miscellaneous modifications to the test cell and the adjacent building.

b. Other Related Annual Costs (FY 1993 dollars)

(1) Facility preparation for operation, research diagnostics and operation costs - The facility is estimated to operate at least for four years with the initial RFP research device installed. The funding needs begin prior to operation at a lower level and reach the level indicated in 1993 and remain approximately level thereafter. The funds are needed for preparation for operation and operations activities such as personnel costs, materials and services, maintenance, spare parts and utilities. Also included are costs for research diagnostics that are required in order to diagnose the plasma conditions within the RFP device.

Mrs. LLOYD. Thank you again for your testimony.
Dr. Overskei, it's your turn.

**STATEMENT OF DR. DAVID OVERSKEI, SENIOR VICE PRESIDENT,
FUSION DIVISION, GA TECHNOLOGIES, INC., WASHINGTON, DC**

Dr. OVERSKEI. Thank you, Madam Chairman. I am David Overskei, Senior Vice President of the Fusion Division of General Atomics, located in San Diego, California, and I wish to thank you all for the opportunity to appear before you today and offer my testimony concerning the U.S. fusion research program.

General Atomics is the largest industrial participant in the U.S. fusion research program. Our fusion program encompasses over 250 scientific, technical, and support personnel, and represents an annual budget of approximately \$36 million. Under contract to the Department of Energy, we have designed, built, and now operate the DIII-D tokamak experimental device, which is the second largest tokamak facility in the country. In addition to doing most of this research ourselves, we have also made our program available to other institutions and national laboratories both abroad and in the U.S., and I am pleased to say that we have participants from 8 universities and 5 of the national labs that are presently working on the research program with us. We have also major research efforts in fusion theory, applied plasma physics, fusion technology and materials development, and we are a major participant in various international fusion research collaborations. Certainly the premier one and which we think is a paramount example of international collaboration is our ongoing collaborative effort with the Japanese Atomic Energy Research Institute, which I will discuss more later.

In addition, we carry out a number of significant fusion research initiatives using our own internal research and development funds. These expenditures have been in excess of \$10 million since 1980.

I want to talk about three things today: first is progress of the program to date; second, next steps as I see them; and, third, funding recommendations.

With regards to progress a great deal has already been said, particularly in the popular press concerning the fusion program's lack of progress. In the 1970s, the program sponsors promised a working fusion reactor by the turn of the century, but clearly we will not achieve that goal. The U.S. Government, through the Department of Energy, has committed millions of dollars each year, yet the progress, though enormous to those in the field, is difficult to quantify to those not in the fusion community. I would like to address these concerns which many of you on this Committee may share.

Let me begin by reminding you that scientific progress cannot be legislated or mandated. This was pointed out quite eloquently by Harold Furth in his testimony. We may be disappointed with the pace. We also know that science succeeds most often in messy fits and starts rather than neat and very predictable increments. The fusion program is first and foremost a scientific research program, and thus subject to the same constraints. The magnitude and the complexity of the problem is evident when one realizes that we are very close to replicating the power of the stars in the laboratory.

Having said that, let me also remind you that many of the promises which now seem so exaggerated were predicated on certain assumptions about funding levels which did not come to pass. Dr. Furth also alluded to that. In fact, funding levels have been less than half of what was assumed in 1976 when planners said a demonstration power plant could be built within 20 years. And more than money we have lost in the intervening decade probably one of our most critical resources: those of the highly trained and skilled personnel who have left fusion for other concerns. This is a concern and consequence not only for the magnetic fusion energy program, but is also something that should concern the U.S. in its efforts to maintain an edge in a leadership position in high technology in all aspects.

Nevertheless, despite the unpredictable nature of science and research and reduced funding levels, the U.S. fusion program has made tremendously significant progress toward our goal of understanding and recreating the power of the sun on Earth.

One measure of our progress, and perhaps the most readily appreciated, has been the excellent results we have achieved this past year from the various experimental devices within the U.S. program. There are a number of such items of progress, but I am not going to go through them all. I'm going to cite just a few examples. Those would include the 6.8 percent beta on the DIII-D device at General Atomics. This is at present the highest beta reached in any tokamak device in the world, beta being the ratio of plasma pressure to the strength of the field needed to confine it, and the consequence is that the higher the beta, the more cost effective an eventual reactor would be. Now, as a point of a standard, reactor designers have long felt that a beta of at least 5 percent was a minimum. So you can see that we have already done substantially better than that. And we are glad to see that Harold's PBX-M program is close but nonetheless behind us.

Moreover, the scientists at General Atomics have demonstrated 2 modes of improved performance, and it should be noted that the program on TFTR has also identified a significant mode of improved performance as well. With further research and a physics understanding of this improved operating condition, we can reduce the cost of future devices without compromising performance. In addition, the TFTR at Princeton has achieved a record temperature, as already cited, of about 30 kilovolts, which is in excess of 300 million degrees, again well above the levels we believe are needed for a power-producing reactor.

Another measure of progress is $NT\tau$, which is the product of the 3 parameters of density, temperature, and energy confinement time; in other words, the time scale in which we can hold this heat in place, which is needed to achieve an energy-producing, self-sustaining fusion reaction. In the past 10 years alone, the tokamak has demonstrated a thousandfold improvement in this critical performance criterion. What is even more encouraging is that we are within a factor of 10 of achieving the levels required for self-sustaining fusion reaction.

Our success in attaining these exciting results is very gratifying, but an even more important indicator of progress is the tremendous increase in our understanding of what is going on inside of

those devices and our ability to accurately predict how the plasma and the experiments will perform. This is crucial for the successful design of future machines. The more we understand about the physics of what is actually happening, the better able we are to design machines that perform the way we want them and without costly overdesign. This is one of the major reasons for the redirection which Dr. Hunter had discussed earlier.

One good example of this kind of progress can be seen in General Atomics high beta research efforts. In the 1970s, our theorists developed models that said it would be possible to obtain high beta operation by increasing the shaping of the plasma. We developed a program of plasma-shaping research at General Atomics which included carefully planned hardware upgrades, the addition of newly specialized detection and diagnostic instruments, and a program of experiments to test the theoretical models. We have been accumulating data ever since, as well as other programs both in the U.S. and abroad, data which not only supports our models and in some cases indicates that some of the models were incorrect and therefore were improved upon, but has allowed us to expand our models and go even further. We have not only achieved a better than 5 percent beta, we now feel that we know the physics of the processes which define the limits and we feel we now know how to optimize machines, present and future machine designs, for even higher beta performance. This has tremendous implications for designing and building future devices such as the CIT, and eventually fusion test reactors.

With regards to the next step, where does the fusion program go from here? Clearly, we must have our first priority as continuing to operate the machines and facilities we have, continuing to build the base of data that will lead to full understanding and the path to a demonstration of a burning plasma. Present machines not only represent an enormous investment, they are capable of producing results that will advance the state of the art for years to come, particularly some of the more flexible ones such as the DIII-D at General Atomics.

We will need to continue to upgrade the performance of these machines through the addition of new types of heating systems which need to be tested to verify physics models and confirm performance predictions. But these upgrades represent a fraction of the original capital cost. We must continue to leverage our existing investment to ensure that we realize the full potential of our present experimental facilities and the investment of the taxpayer's money which they represent.

At the same time, we must look ahead to the next stage: operating with ignited or burning plasmas. Ignition is significant because it means that fusion reactions have become self-sustaining. It will continue—the fusion reaction would continue without the requirement of additional heating from external sources. Unfortunately, it is not possible to operate any of our existing machines at ignition levels. They simply aren't designed to attain those conditions. Thus, we need a national program with full participation by all the experts in the U.S. fusion community and a new facility. At the heart of this new facility would be a new device that would for the

first time generate a controlled and sustained fusion reaction on Earth; in effect, a miniature sun in the laboratory.

The CIT was envisioned as this device, but in the 3 years since the conceptual design for the CIT began there have been enormous advances and additions to the fusion data base in addition to developments in the fusion theory. The present empirical data base defines an operating window within which the goal of a burning plasma could be realized. However, the extrapolation of this data base leads to 2 very different devices. One, based on conservative data, is a large machine with much flexibility, and a high probability of reaching burning plasma conditions, but also higher costs than the presently projected CIT. The other, based on the best results to date and the best machine performance to date, would probably be a much smaller, and hence less expensive, device; however, less flexible and with a small probability of attaining burning plasma conditions.

The selection of the correct design is clearly crucial and requires significant understanding of the physics of the transport in these extremely high-temperature regimes. This, essentially, is the goal of the present DOE program redirection: to better understand the science and therefore develop theoretical models based upon which we can corroborate the large data base and then predict the future. To facilitate this process, we feel that the current requested \$5 million for CIT construction funding should be diverted instead to carry out additional research on the CIT design and the physics modeling. This is especially important since a new and very innovative engineering design concept has just recently been proposed and we need time to develop it more fully. This is the same design that Dr. Furth alluded to just moments ago.

Within approximately a year, we should have a much improved CIT design, one that capitalizes on the progress we have made in our understanding of these important issues of transport and high betas. We will also have developed a physics model consistent with our large and growing base of data which can be used to more accurately predict CIT parameters and performance, thus optimizing performance for the minimum required investment. At that time we should aggressively proceed with construction.

In addition to the CIT or a burning plasma initiative there would be another area which I feel has been overlooked in the U.S. fusion program to date; that is, development of the necessary low activation materials that really give the fusion program the answer to being a better machine or a better concept for long-term energy production than fission. It would allow us to demonstrate and realize the low activation, and therefore the lower volume and lower level of radioactive waste, which is a distinct advantage that we would offer over other long-term energy alternatives.

A third area of focus for the U.S. fusion program would be our continuing role as an international leader in fusion research. Just as with high-energy physics or cancer research, our subject is too complex for any single scientist or any single lab to be able to exploit and therefore divine and accomplish fully. We need to leverage off results and accomplishments not only in the various U.S. research centers, but in those abroad as well. Essentially, the U.S.

should participate but should take advantage of a lot of the contributions and efforts in the international community as well.

One example is the ITER process, a U.S./Japan/European/USSR collaboration on the design of an International Thermonuclear Experimental Reactor. The many research initiatives developing data to support the design are very important. Other examples of international cooperation again come from GA. The DIII-D facility represents a \$70 million collaborative—\$70 million collaborative effort with the Japanese Atomic Energy Research Institute. They have helped fund major upgrades as well as operation of the machines since 1979, and they continue to participate in the DIII-D program until at least 1992.

We also have a standing agreement with the Germans and with the Joint European Tokamak, which is at Culhan where we are presently in the process of doing collaborative experiments to define and better illustrate the confinement processes in large-scale tokamaks. We have also had ongoing interactions and exchanges with scientists from the Soviet Union, where they come into our facility and, similarly, we go to their facility to accelerate the research program in specific areas of common interest.

With regards to funding, we do need to accomplish—what do we need to accomplish these things which have already been mentioned? The simple answer is that we need more money if we are to achieve the best science and the best progress possible. But the funding question is not a simple one, as you are well aware. Mindful of the constraints upon you, I urge you to consider funding the fusion research program during fiscal '90 at at least a level equal to this year's budget. This will allow us to continue to realize the full potential of our base programs, and allow us to continue to play a key role in the international fusion research efforts.

Furthermore, as regards funding for the CIT, I urge you to defer funding for construction for fiscal '90, and instead allow us to apply those monies to optimize the physics and the design for the best experiment possible. I believe you will agree that this is a prudent and wise use of taxpayers' dollars and will ultimately yield a greater return on our investment, while still permitting us to demonstrate a controlled burning plasma before the turn of the century.

Please do not interpret my remarks as indicating any lack of support for CIT. Far from it. CIT, in my perspective, is a vital element of our overall program, and when the time is right, when we have an optimized design and a strong theory and experimental data base, then I will ask you to provide the full funding needed for the CIT. It may be a sizable amount, potentially requiring an increase of up to \$120 million per year above the present levels, if we are to accommodate the base program and realize the goal of the CIT in a reasonable time scale. But on that day when I ask for your support it will be with the full confidence that we are asking you to spend that money wisely on an investment which we are confident will perform to all of our expectations, demonstrating for the first time controlled thermonuclear fusion.

In closing, let me just say a few words concerning the value of the fusion program. Many others have already addressed this, so I will try and keep my remarks brief. You have already heard, doubtlessly repeated many times, that fusion represents a key ele-

ment of a long-range energy policy for the U.S. and, indeed, for the world—a nonfossil fueled, environmentally benign, long-term source of energy. This is quite true, but it will be decades before this potential is fully realized.

Does this mean that fusion has nothing to offer in the short term? The answer is emphatically no. The fusion program represents U.S. scientific research and development at its finest. Fusion research, no less than high-energy physics or space science, advances man's understanding of the world around him. On a less abstract plane, the fusion program is also helping keep America's technological edge alive. It is a training ground for scientists and engineers in fields as varied as superconductivity and high-power microwaves. It contributes daily to advances that are changing the face of U.S. technology. Finally, and perhaps most importantly, it can provide inspiration to the next generation of students to aspire to a career in science, and take on the commitment to hard work that that entails.

I would also like to put our requested funding of \$350 million in perspective. It is worth noting that this funding level is only approximately twice that of what the Department of Defense spends on military bands each year, and is comparable to the amount of money that the American public spends on soft drinks in a matter of about 3 days.

Lastly, our effort to identify and obtain controlled thermonuclear fusion research is an effort which will basically give us an insurance policy to the future. Each and every one of us buy insurance policies, not to take care of ourself, but to take care of those which we love, which tends to be our family and our grandchildren and our posterity. Well, we are consuming the fossil fuels for our grandchildren now, and we owe them more than the .03 percent investment in fusion energy that we have at present as an insurance policy into securing their future with having a free society that has essentially guaranteed sources of energy.

I want to thank you, Madam Chairman, and Committee Members for your longstanding support of the U.S. fusion program. I also want to urge your continued support, recognizing that the modest investment you make today has the potential to yield benefits of incalculable value both in terms of energy for the next centuries and in terms of more near-term scientific benefit. Thank you.

[The prepared statement of Dr. Overskei follows:]

STATEMENT FOR THE RECORD

**SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT
COMMITTEE ON SPACE, SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES**

**FY90 DOE AUTHORIZATION
MAGNETIC FUSION ENERGY**

**DR. DAVID O. OVERSKEI
SENIOR VICE PRESIDENT
FUSION DIVISION
GENERAL ATOMICS**

FEBRUARY 21, 1989

I am David O. Overskei, Senior Vice President of the Fusion Division of General Atomics, located in San Diego California. Thank you for the opportunity to appear before you today and offer my testimony concerning the U.S. fusion research program.

General Atomics and describe the scope of our fusion research program. General Atomics is the largest industrial participant in the U.S. fusion research program. GA's fusion program encompasses over 250 scientific, technical, and support personnel and represents an annual budget of \$36 million. Under contract to the DOE, we designed, built, and now operate the DIII-D tokamak experimental device, the second largest tokamak facility in the country. Major research efforts include fusion theory, applied plasma physics, fusion technology and materials development, and we are a major participant in various international fusion research collaborations. In addition, we carry out a number of significant fusion research initiatives using our own internal research and development fund, these expenditures have been in excess of \$10M since 1980.

I want to talk about three things today: progress of the program to date, the next steps as I see them, and funding recommendations.

Progress

First, progress. A great deal has been said, particularly in the popular press concerning the fusion program's lack of progress. In the 1970s, the program sponsors promised a working fusion reactor by the turn of the century, but we clearly we will not achieve that goal. The U.S. government, through the DOE, has committed millions of dollars each year, yet the progress, though enormous to those in the field, is difficult to quantify to those not in the fusion community. I would like to address these concerns, which many of you on this Committee may share.

Let me begin by reminding you that scientific progress cannot be legislated or mandated; that is the very nature of scientific research. While we may be disappointed with the pace, we also

know that science succeeds most often in messy fits and starts rather than neat, predictable increments. The fusion research program is first and foremost a scientific research program, and thus subject to the same constraints. The magnitude and complexity of the problem is evident when one realizes that we are very close to replicating the power of the stars in the laboratory.

Having said that, let me also remind you that many of the promises which now seem so exaggerated were predicated on certain assumptions about funding levels which did not come to pass. In fact, funding levels have been less than half of what was assumed in 1976 when planners said a demonstration power plant could be built within 20 years. And more than money has been lost in the intervening decade; we have also lost an even more important asset, the highly trained and skilled personnel who have left fusion for other careers.

Nevertheless, despite the unpredictable nature of scientific research and reduced funding levels, the U.S. fusion program has made tremendously significant progress toward our goal of understanding and recreating the power of the sun on earth.

One measure of our progress, and perhaps the most readily appreciated, is the excellent results we have achieved this past year from the various experimental devices within the U.S. program. These include, for example, achievement of a 6.8% beta on the DIII-D device at General Atomics, the highest beta reached of any tokamak device in the world. (Beta is the ratio of plasma pressure to the strength of the field needed to confine it. The higher the beta, the more cost-effective the reactor. Reactor designers have long felt that a beta of at least 5% was a minimum, so you can see we have already done better than that.) Moreover, the scientists at General Atomics have demonstrated two modes of improved performance. With further research and a physics understanding of this improved operating condition, we can reduce the cost of future devices without compromising performance. In addition, the TFTR at Princeton, has achieved a record temperature of about 30 keV (330 million degrees), again well above the levels we believe are needed for a power-producing reactor.

Another measure of progress is $nT\tau$ --the product of the three parameters (density, temperature, and energy confinement time) needed to achieve an energy-producing, self-sustaining fusion reaction. In the past ten years alone, the tokamak has demonstrated a thousandfold improvement in this critical performance criterion. What is even more encouraging is that we are within a factor of ten of achieving the levels required for self-sustaining fusion reaction.

Our success in attaining these kind of exciting results is very gratifying of course, but an even more important indicator of progress is the tremendous increase in our understanding of what is going on inside those devices-- and our ability to accurately predict how they will behave. This is crucial for the successful design of future machines. The more we understand about the physics of what is actually happening, the better able we are to design machines that perform the way we want them to, without costly over-designing.

One good example of this kind of progress can be seen in GA's high beta research efforts. In the 1970s, our theorists developed models that said it should be possible to obtain high beta operation by increasing the shaping of the plasma. We developed a program of plasma-shaping research at GA, which included carefully planned hardware upgrades, the addition of new specialized detection and diagnostic instruments, and a program of experiments to test these models. We have been accumulating data ever since--data which not only supports our models, but has allowed us to expand our models and go even further. We have not only achieved a better than 5% beta, we know the physics of the process which defines the limits, and we know how to optimize machine designs for even higher beta. This has tremendous implications for designing and building future devices and eventual fusion test reactors.

Next Steps

Where does the fusion program go from here? Clearly, we must have as our first priority continuing to operate the machines and facilities we have, and continuing to build the base of data that will lead to full understanding and the path to demonstration of a burning plasma. Present machines not only represent an enormous

investment, they are capable, of producing results that will advance the state of the art for years to come, particularly the most flexible ones such as GA's DIII-D. We will need to continue to upgrade the performance of these machines through the addition of new types of heating systems which need to be tested to verify the physics models and confirm performance predictions. But these upgrades represent a fraction of the original capital cost. We must continue to leverage our existing investment, to ensure that we realize the full potential of our present experimental facilities and the investment of the taxpayer's money which they represent.

At the same time, we must look ahead to the next stage--operating with ignited or burning plasmas. Ignition is significant because it means that the fusion reaction has become self-sustaining--it will continue without the requirement of additional heating from external sources. Unfortunately, it is not possible to operate any of our existing machines at ignition levels--they simply aren't designed to attain those conditions. Thus, we need a national program with full participation by all the experts in the U.S. fusion community, and a new facility. At the heart of this new facility would be a new device that would--for the first time--generate a controlled and sustained fusion reaction on earth, in effect a miniature sun in the laboratory.

CIT was envisioned as this device, but in the three years since the conceptual design for the CIT began, there have been numerous advances and additions to the fusion data base. The present empirical data base defines an operating window within which the goal of a burning plasma could be realized. However, extrapolation of this data base leads to two very different devices. One, based on conservative data, is a large machine with much flexibility and a high probability of reaching burning plasma conditions, but also higher costs than the presently projected CIT. The other, based on the best results to date, would probably be a much smaller and hence less expensive device, however with a small probability of attaining a burning plasma.

Selection of the correct design is clearly crucial, and requires significant understanding of the physics of transport in these

extremely high temperature regimes. This, essentially, is the goal of the present DOE program redirection. To facilitate this process, we feel that the currently requested \$5 million for CIT construction funding be diverted instead to carry out additional research on the CIT design, and physics modelling. This is especially important since a new and very innovative engineering design concept has just recently been proposed and we need time to develop it more fully.

Within approximately a year, we should have a much improved CIT design, one that capitalizes on the progress we have made in our understanding of these important issues of transport and high betas. We will also have developed a physics model consistent with our large and growing base of data, which can be used to more accurately predict CIT parameters and performance, thus optimizing performance for the minimum required investment. At that time we should aggressively proceed with construction.

The third area of focus for the U.S. fusion program should be continuing our role as an international leader in fusion research. Just as with high energy physics or cancer research, the subject is too complex for any single scientist or single lab to be able to exploit fully. We need to leverage off results and accomplishments not only in the various U.S. research centers, but in those abroad as well.

One example is the ITER process, a U.S./Japan/European/USSR collaboration on the design of an International Thermonuclear Experimental Reactor (ITER). The many research initiatives developing data to support the design, are very important. Another example of international cooperation comes again from GA. The DIII-D facility represents a \$70 million collaborative effort with the Japanese Atomic Energy Research Institute. They have helped fund major upgrades as well as operation of the machine since 1979, and will continue to participate in the DIII-D program until at least 1992.

Funding

What do we need to accomplish these things? The simple answer is we need more funding if we are to achieve the best science, and the

best progress possible. But the funding question is not a simple one, as you are so well aware. Mindful of the constraints upon you, I urge you to consider funding the fusion research program during FY90 at at least a level equal to this year's budget. This will allow us to continue to realize the full potential of our base programs, and allow us to continue to play a key role in the international fusion research efforts.

Furthermore, as regards funding for the CIT, I urge you to defer funding for construction for FY90, and instead allow us to apply those monies to optimize the physics and the design for the best experiment possible. I believe you will agree that this is a prudent and wise use of taxpayer dollars, and will ultimately yield greater return on our investment, while still permitting us to demonstrate a controlled burning plasma before the turn of the century.

Please do not interpret my remarks as indicating any lack of support for the CIT. Far from it. CIT is a vital element of our overall program, and when the time is right, when we have an optimized design, with a strong theory and experimental data base, then I will ask you to provide the full funding needed for CIT. It will be a sizeable amount, potentially requiring an increase of up to \$120 million per year above the present level, but on that day when I ask for your support, it will be with full confidence that I am asking you to spend that money wisely, on an investment which we are confident will perform to expectations-- demonstrating for the first time controlled, sustained thermonuclear fusion.

In closing, let me just say a few words concerning the value of the fusion program. Many others will address this subject, so I will keep my remarks brief. You have all heard, doubtless repeatedly, that fusion represents a key element of a long-range energy policy for the U.S. and indeed the world--a non-fossil fueled, environmentally benign, long-term source of energy. And this is quite true, but it will be decades before this potential is realized.

Does this mean that fusion has nothing to offer in the short-term? The answer is emphatically no. The fusion program represents U.S. scientific research and development at its finest. Fusion research,

no less than high energy physics or space science, advances man's understanding of the world around him. On a less abstract plane, the fusion program is also helping keep America's technological edge alive. It is a training ground for scientists and engineers in fields as varied as superconductivity and high power microwaves. It contributes daily to advances that are changing the face of U.S. technology. Finally, and perhaps most importantly, it can provide inspiration to the next generation of students to aspire to a career in science, and take on the commitment to hard work that that entails.

I want to thank you Madame Chairman and committee members for your long-standing support of the U.S. fusion program. I also want to urge your continued support, recognizing that the modest investment you make today has the potential to yield benefits of incalculable value, both in terms of energy for the next centuries, and in terms of more near-term scientific benefit. Thank you.

Mrs. LLOYD. Thank you, Dr. Overskei.

Dr. Parker, we look forward to your testimony at this time.

STATEMENT OF DR. RONALD R. PARKER, DIRECTOR, PLASMA FUSION CENTER, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, CAMBRIDGE, MA

Dr. PARKER. Thank you, Madam Chair. Being the last speaker in this panel, I'm reminded of Sergeant Preston's Law of the Yukon. Perhaps you've heard it, but it goes like this: The scenery only changes for the lead dog. So I'm afraid that I will be repeating some of what you've already heard, but I will try to give it a special emphasis.

Mrs. LLOYD. Well, we want your special touch and your expertise, and that's the reason you're on this panel.

Dr. PARKER. Thank you.

First of all, I do very much appreciate your invitation to convey my views on the objectives and priorities of the U.S. fusion program.

Fusion energy has long been considered a nearly ideal source of energy—abundant, inexpensive fuel; low environmental impact; and a cost of electricity competitive with that of a fission plant. Like fission, it is a nuclear process and possesses the advantages of a high-power density system. However, unlike fission, the reaction products are nonradioactive, and a fusion power plant would therefore generate vastly lower amounts of radioactive waste than its fission-based counterpart. Although the country must, in my view, increasingly rely on fission in the short term, a shift in the long term to a fusion-based power system would be the most attractive choice of all possible future energy options for baseload power generation.

While the technical difficulties for making a fusion-energy reactor are great, enormous technical progress has been made. Temperatures in excess of those required for fusion have already been produced in our country's largest experiment, TFTR. You have heard these results described by previous speakers. I'll add my own twist by pointing out that these temperatures are some 20 times those within the sun.

You've also heard that the $Tn\tau$, the single most important figure of merit for fusion, has been significantly advanced; and, in fact, it's increased by a factor of 10,000 since I entered the field some 25 years ago. This parameter is now within one-tenth of what would be required for a deuterium-tritium reactor, and one one-hundredth of that, of what would be required for a pure deuterium reactor. The past rate of progress leads me to believe that fusion energy's promise, which is more appealing now than ever before, will be realized if, and only if, a sustained commitment comparable to that expended in the last decade in fusion research is made.

Now, in order to continue to make progress toward the realization of fusion, a number of near-term objectives must be pursued. And each of us working in fusion assign somewhat different priorities to each of those objectives, but, in fact, there is very little disagreement on the collective set of goals. I will focus my remarks on those facets of the fusion program which I currently view as the

most important and deserving of strong support, and these are: understanding and controlling transport or confinement, continuing the design and supporting R&D of a compact ignition device, and conducting basic R&D and technology and materials areas which have critical impact on the attractiveness of a fusion reactor.

First, with regard to the confinement issue, the mechanism by which heat and particles are transported across the lines of magnetic force in a toroidal confinement device is a classic problem in plasma physics, the branch of science which provides the underpinning for fusion. Elaborate theoretical models have been developed which describe the flows resulting from the chaotic state produced by various forms of microscopic instability. Also, sophisticated measurement tools have been developed, both by fusion researchers and others, which can measure this turbulence and confirm or deny that it is responsible for the particle energy loss.

However, due to the pressure to push parameters to the new frontiers, as in the very successful TFTR experiments during the past year, it has not been possible to fully invest the time and resources into the effort which is required to quantitatively test the relationship of theory and experiment. I might point out that it is between that basic process of confrontation that progress is made in science.

As a result, we have had to rely on purely empirical models to predict the performance of future devices, and this has proven to be a generally unsatisfactory approach since such models, as you have heard today, are useful for interpolation—that is, for systems whose parameters lie within the range already studied—but not extrapolation.

I became acutely aware of this problem in designing the CIT in my capacity as head of the physics team. In response to this effort, or this problem, I and others urged the Department of Energy, Office of Fusion Energy, to mount a serious effort to resolve this most fundamental and critical issue. Subsequently, this Tokamak Task Force was established with the mission of organizing the manpower and resources necessary to advance our understanding. The DOE Director of Energy Research, as you've heard today, Dr. Robert Hunter, has been extremely supportive of this effort and committed to it. And he evidently shares my opinion and that of many of my colleagues nationwide that obtaining a predictive understanding of confinement should be a very high-priority near-term goal of the program.

There are many aspects to attacking this problem: improved theory supported by computation; development of new, highly sophisticated diagnostics; construction of small-scale devices which can be used in a paradigmatic—I knew I would have a problem with that word—fundamental physical—paradigmatic way, I'm sorry, fundamental physical processes. Increased run time must also be made available on our presently operating major facilities; namely, TFTR, DIII-D, TEXT, and ATF, so that new ideas can be freely and fully tested. Following this approach, I am optimistic that a quantitative picture of toroidal transport will soon begin to emerge, and understanding toroidal transport should remain in the near term a very high-priority objective in the U.S. fusion effort.

I turn now to the issue of building an ignition device. I have felt strongly for many years that constructing and operating a compact ignition device should be a very high-priority objective for the U.S. fusion program. In fact, I would like to quote my testimony presented to this subcommittee 4 years ago, and I quote:

I believe that achieving and studying the ignited plasma state is an absolutely essential and correct next step for the Magnetic Fusion Energy program. I also believe that expecting to accomplish this goal in a tokamak of modest size, high field, tight time scale, and modest cost is reasonable and that we should proceed in this direction as soon as possible.

In the intervening time, my conviction that this initiative is the correct one for the fusion program has only been strengthened.

However, a practical difficulty, one which I am sure you and your committee members, Madam Chair, will recognize, has emerged; namely, lack of adequate resources to do the job. At the time of my 1985 testimony, the President's budget for fusion was \$390 million, down from \$437 million in FY 1985. As you've heard today, the President's budget for this coming fiscal year is \$349 million, virtually the same as for last year. This represents a decrease of over \$100 million, including and accounting for inflation, from the FY 1985 level. It should be obvious that the fusion program cannot undertake an ignition experiment within this proposed budget without suffering serious damage to the pursuit of other objectives.

The program therefore now finds itself confronted with a conundrum. The knowledge exists to build and operate a highly cost-effective ignition test device which would produce invaluable information pertaining to the design of a reactor; however, because of the budget climate this vital step cannot be initiated without sacrifice of other important objectives. My view is that some compromise will be required, both on the part of the fusion program, which will be forced to delay or even abandon progress in lower priority areas, and on the part of Government, which must recognize the long-term benefit to the Nation in maintaining momentum and world leadership in important scientific and technical areas such as fusion.

The present CIT machine design is a good compromise between competing demands of obtaining high performance while severely constraining the cost. It is possible that a new design concept mentioned by Dr. Furth and now under study may result in an even more optimized design. A device with the present, maximum design parameters has an excellent chance of reaching ignition, provided full magnet and heating power are supplied and effort on understanding and controlling transport is sustained during the construction phase. Adding full magnet and heating power will require more resources, about \$200 million, than is included in the present estimated project cost.

However, without this additional capability, the probability of ignition in CIT is considered to be low, based on our present knowledge. I might add that should we be so lucky that the extra capability which I'm advocating was not needed, this extra capability would be very effectively used in a number of ways; for example, by beginning experimentation with advanced fuels.

Now a compact ignition device is envisioned to be the central focus of the fusion program during the 1990s. Budgets are likely to be highly constrained during this period, implying that the physics and engineering design must be highly optimized and the operation lean and efficient. This will require making full use of the intellectual and capital resources of our program, which mandates, to my way of thinking, organizing this effort as a national project. And a special effort must be made to involve universities, as well as all of the major fusion laboratories. While not without precedent in other communities, this would represent a new mode of operation for the U.S. fusion program.

With the above remarks as background, I then recommend that Congress, rather than provide construction funds for CIT for FY 1990, provide instead an equivalent amount, \$5.5 million, of operating funds—in operating funds for use in continuing to optimize a CIT design and for carrying out essential R&D. And I further recommend that the DOE and fusion program leadership resolve during this coming fiscal year to accomplish the following:

(a) Identify and commit to a construction funding pattern which will permit building a device with good probability of ignition at the beginning of its operating phase; and

(b) Establish an ignition project management structure which reflects a national undertaking.

Regarding item (a), incremental fusion program funding will be required, probably in the amount of \$30 to \$50 million annually, if draconian impact on other program objectives is to be avoided. However, assuming satisfactory progress on these issues, we would then be in an excellent position to embark on construction of an ignition device beginning in FY 1991.

Earlier in my remarks, I referred to the promise of fusion as a moderate-cost, low-environmental-impact, nearly ideal source of energy. In addressing fusion's near-term objectives, it is important not to lose sight of our long-term goal. Thus, I come to the last objective which is of high priority in my vision of the program: maintaining a vigorous materials and technology effort focused on long lead-time R&D in critical areas which truly make a difference in the attractiveness and credibility of our product.

Two examples quickly come to mind: low activation materials with the requisite structural and other properties necessary to build low activation D-T reactors, that is, deuterium-tritium reactors; and high-performance superconductors and high-strength materials which would enable both smaller, higher performance D-T reactors—I believe this is similar to the concept Dr. Hunter mentioned in his testimony—and also advanced fuel, all deuterium, for example, or nearly neutronless deuterium-helium-3 devices. It would, this technology would enable these types of devices to be seriously considered.

Regarding R&D in the magnetics area, research at MIT has indicated that very high performance niobium-tin superconductors can be manufactured by a new powdered metallurgy process. These superconductors possess critical fields and current densities already equal to the current density and field which exist in the copper-magnet design for CIT and therefore has the image of a possible superconducting version, although not in the near future.

More advanced niobium-aluminum conductors made by the same process might have even higher performance. Much work remains to be done to make these conductors practical, and additional R&D must be carried out to develop special high-strength materials required to support the intense magnetic forces. However, by exploiting such high-performance magnet properties we can begin to contemplate truly improved reactor—a truly improved reactor vision. And this is only one example of an R&D area where relatively small investment can pay handsome dividends relating to the attractiveness of our product.

Maintaining long-term critical-area R&D of the type just described is essential for establishing the scientific and technological basis for a realistic assessment of fusion energy's potential. There has been a tendency in the past in our program to sacrifice long-term work in favor of achieving shorter term goals. This erosion of critical-area R&D must be reversed if we are to make a serious commitment to achieving our programmatic goal.

Madam Chair and Committee members, I recognize the very difficult budget climate which we face today, and I also recognize that there are other important programs, one of which we heard about earlier, which have had to endure even larger cuts. I am very grateful to you and to your Subcommittee for your strong support over these difficult past several years, since without your help the fusion program would have been far less effective in making the excellent progress it has produced to date.

At this time, fusion finds itself poised to make impressive new strides as we enter the next decade. However, to do this will require modest new investment, both to undertake the crucial ignition step and to make a new commitment to establish the scientific and technological basis for our product. I am confident that your support will be rewarded by the development of a fundamentally new form of energy, one that future generations will find attractive, useful, and, in all probability, indispensable.

Thank you, Madam Chair.

[The prepared statement of Dr. Parker follows:]

Objectives and Priorities of the US Fusion Program

Testimony presented to

**The Subcommittee on Energy Research and Development
Committee on Science, Space and Technology**

**U.S. House of Representatives
Washington, D. C.**

by

**Dr. Ronald R. Parker
Director, MIT Plasma Fusion Center**

February 21, 1989

Madame Chair, Members of the Subcommittee:

I am Ronald Parker, Director of MIT's Plasma Fusion Center. I am very grateful for your invitation to convey my views on the objectives and priorities of the US fusion program.

Fusion energy has long been considered a nearly ideal source of energy - abundant, inexpensive fuel; low environmental impact; and a cost-of-electricity competitive with that of a nuclear fission plant. Like fission, it is a nuclear process and possesses the advantages of a high power density system; however, unlike fission, the reaction products are non-radioactive and a fusion power plant would therefore generate vastly lower amounts of radioactive waste than its fission-based counterpart. Although the country must, in my view, increasingly rely on fission in the short term, a shift in the long term to a fusion-based power system would be the most attractive choice of all possible future energy options for baseload power generation.

While the technical difficulties in making a fusion-energy reactor are great, enormous technical progress has been made. Temperatures in excess of those required for fusion ($100,000,000^{\circ}\text{C}$) have already been produced in our country's largest fusion experiment, the TFTR tokamak at Princeton. The single most important figure-of-merit which characterizes fusion progress (density-temperature-confinement-time product or $nT\tau$) has been increased by ten thousand, since I entered the field some twenty-five years ago. This parameter is now within one tenth of that required for a deuterium-tritium reactor, and one hundredth of that needed for a pure deuterium device. The past rate of progress leads me to believe that fusion energy's promise, which is more appealing now than ever before, will be realized if, and only if, a dedicated effort comparable to that expended in the last decade is sustained.

In order to continue to make progress toward the realization of fusion energy, a number of near-term objectives must be pursued. While each of us working in fusion research would assign somewhat different priorities to

reaching various objectives, there is very little disagreement on the appropriate set of goals. I will focus my remarks on these facets of the fusion program which I currently view as the most important and deserving of strong support. These are: understanding and controlling toroidal transport (the flow of heat and particles across toroidal magnetic lines of force); continuing the design of a compact ignition device, and carrying out supportive scientific and technical R&D; and conducting basic R&D in technology and materials areas which have critical impact on the attractiveness of a fusion reactor.

Understanding and Controlling Toroidal Transport

The mechanism by which heat and particles are transported across the lines of magnetic force in a toroidal confinement device is a classic problem in plasma physics, the branch of science which provides the underpinning for fusion development. Elaborate theoretical models have been developed which describe the flows resulting from the highly chaotic state produced by various forms of microscopic instability. Highly sophisticated measurement tools have been developed, both by fusion researchers and workers in other fields, which can measure this turbulence and confirm (or deny) that it is responsible for energy and particle loss. However, due to the pressure to push parameters to new frontiers, as in the very successful TFTR experiments this past year, it has not been possible to invest the time and resources into the effort required to quantitatively test the relationship of theory and experiment. (It is from this classic process of confrontation that scientific progress is made). As a result, we have had to rely on purely empirical models to predict the performance of future planned devices, including a reactor. This has proven to be a generally unsatisfactory approach since such models are useful for interpolation (that is, for systems whose parameters lie within the range already studied), but not extrapolation.

I became acutely aware of this problem in designing the Compact Ignition Tokamak (CIT) in my capacity as head of the physics team. In response to this problem, I and others urged the Office of Fusion Energy to mount a serious effort to resolve this most fundamental and critical issue. Subsequently, the Transport Task Force was established with the mission of organizing the manpower and resources necessary to advance our understanding in this area. The DoE Director of Energy Research, Dr. Robert O. Hunter, Jr., has been

extremely supportive of this effort, and evidently shares my opinion and that of many of my colleagues nationwide that obtaining a predictive understanding of transport should be a very high-priority near-term goal of the fusion program.

There are many aspects to attacking this problem: improved theory supported by computational simulations; development of new, highly sophisticated diagnostics; and construction of small-scale devices which can be used to isolate, in a paradigmatic way, fundamental physical processes. Increased run-time must also be made available on our presently operating major facilities (TFTR, DIII-D, TEXT, ATF) so that new ideas can be freely and fully tested. Following this general approach, I am optimistic that a quantitative picture of toroidal transport will soon begin to emerge. In consequence, we will be much better able to optimally design the remaining major experimental steps required for development of a fusion reactor. Thus, understanding toroidal transport should remain, in the near term, a very high priority objective of the US fusion effort.

Achieving and Studying Ignited Plasmas

Ignition refers to the process by which a high-temperature plasma sustains itself while producing energy via fusion reactions. All designs of fusion reactors using magnetic confinement involve plasmas at or near ignition. It follows that achieving ignition, and learning about the properties of ignited plasmas, is essential to producing a viable fusion reactor.

I have felt strongly, for many years, that constructing and operating a compact ignition device should be a very high priority objective for the US fusion program. In fact, I would like to quote my testimony presented to this subcommittee four years ago:

"I believe that achieving and studying the ignited plasma state is an absolutely essential and correct next step for the Magnetic Fusion Energy Program. I also believe that expecting to accomplish this goal in a tokamak of modest size (major radius not larger than two meters), high-field (8-10 Tesla), tight time scale (not more than six years' construction time) and modest cost is reasonable and that we should proceed in this direction as soon as possible."

In the intervening time, my conviction that this initiative is the correct one for the fusion program has only been strengthened.

However, a practical difficulty, one which I am sure you and your committee members will recognize, has emerged: lack of adequate resources to do the job. At the time of my 1985 testimony, the President's budget for fusion was \$390M, down from \$437M in FY 1985. This year, the President's budget is \$349M, virtually the same as for the last year. This represents a decrease of over \$100M, including inflation, from the FY 1985 level. It should be obvious that the fusion program cannot undertake an ignition experiment within this proposed budget without suffering serious damage to the pursuit of other objectives.

The US fusion program now finds itself confronted with a critical issue: the knowledge exists to build and operate a highly cost-effective ignition-test device which would produce invaluable information pertinent to the design of a fusion reactor; however, because of the budget climate, this vital step cannot be initiated without sacrifice of other important objectives. My own view is that compromise will be required, both on the part of the fusion program, which will be forced to delay or even abandon progress in lower-priority areas, and on the part of Congress, which should recognize the long-term benefit to the nation of maintaining momentum and world leadership in important scientific and technical areas such as fusion.

The President's FY 1990 budget request includes a \$5M expenditure for construction of the Compact Ignition Tokamak (CIT), an ignition-test device being proposed for construction and operation and the Princeton Plasma Physics Laboratory. I would like to offer a few comments concerning this specific proposal.

The present CIT machine design is a good compromise between the competing demands of obtaining high performance while severely constraining the cost. It is possible that a new design concept, now under study, may result in an even more highly optimized design. A device with the present, maximum design parameters has an excellent chance of reaching ignition, *provided full magnet and heating power are supplied, and effort on understanding and improving transport is sustained during the early construction phase.* Adding full magnet and heating power capability will require more resources (about \$200M) than is included in the present estimated project cost. Without this

additional capability, the probability of ignition in CIT is considered to be low, based on present knowledge. (A consensus statement supporting these conclusions has been recently issued by MFAC Panel XXII.)

A compact ignition device is envisioned to be the central focus of the national fusion effort during the 1990's. Budgets are likely to be highly constrained during this period, implying that the physics and engineering design must be highly optimized and the operation lean and efficient. This will require making full use of the intellectual and capital resources of the fusion program, which mandates organizing the project as a national effort. A special effort must be made to involve the universities, as well as all of the major fusion laboratories. While not without precedent in other communities, this would represent a new mode of operation for the US fusion program.

With the above remarks as background, I recommend that Congress, rather than provide construction funds for the CIT for FY 1990, provide instead an equivalent amount, \$5M, of operating funds for use in continuing to optimize the CIT design and for carrying out essential R&D. I further recommend that the DoE and the fusion program leadership resolve, during FY 1990, to accomplish the following:

- Identify and commit to a funding pattern during the construction years which will permit building a compact ignition device with good probability of ignition at the beginning of its operating phase;
- Establish an ignition-project management structure which reflects a national fusion program undertaking.

Regarding the first item, incremental fusion program funding will be required, probably in the amount of \$30-\$50 M annually, in order to avoid draconian impact on other program objectives. Assuming satisfactory progress on these issues, I believe that we will be in an excellent position to embark on construction of an ignition device beginning in FY 1991.

Support of Critical-Area R&D

Earlier in my remarks, I referred to the promise of fusion as a moderate-cost, low-environmental-impact, nearly ideal source of energy. In addressing fusion's near-term objectives, it is important not to lose sight of our overall goal. Thus, I come to the last objective which is high-priority in my vision of the fusion program: maintaining a vigorous materials and technology effort focused on long-lead-time R&D in critical areas which truly make a difference in the attractiveness and credibility of our product.

Two examples quickly come to mind: low activation materials with the requisite structural and other properties necessary to build low-activation deuterium-tritium (D-T) reactors; and high-performance superconductors and high-strength materials which would enable both smaller, higher performance D-T reactors and advanced fuel (all deuterium, or nearly neutronless deuterium-helium three) devices to be seriously considered.

Regarding R&D in the magnetics area, research at my institution has indicated that very high performance niobium-tin superconductors can be manufactured by a new powdered-metallurgy process. These superconductors possess critical fields and current densities already equal to the field and current density which exist in the copper-magnet design of the CIT. More advanced niobium-aluminum conductors made by the same process may have even higher performance. Much work remains to be done to make these conductors practical, and additional R&D must be carried out to develop the special high-strength materials required to support the magnetic forces. Exploiting such high-performance magnet properties can give rise to greatly enhanced reactor capability. This is only one example of an R&D area where relatively small investment can pay handsome dividends relating to the attractiveness of our product.

Maintaining long-term critical-area R&D of the type just described is essential for establishing the scientific and technological basis for a realistic assessment of fusion energy's potential. There has been a tendency in the past to sacrifice long-term work in favor of achieving shorter-term goals. The erosion of critical-area R&D must be reversed if we are to make a serious commitment to achieving our programmatic goal.

Madame Chair and Committee members, I recognize the very difficult budget climate which we face today, and I also recognize that there are other important programs which have had to endure even larger cuts. I am very grateful to you and your subcommittee for your strong support over these difficult past several years, since without your help, the fusion program would have been far less effective in producing the excellent progress which it has produced to date.

At this time, fusion finds itself poised to make impressive new strides as we enter the next decade. However, to do this will require modest new investment, both to undertake the crucial ignition step and to make a new commitment to establish the scientific and technological basis for our product. I am confident that your support will be rewarded by the development of a fundamentally new form of energy, one that future generations will find attractive, useful, and in all probability, indispensable.

PANEL 1 DISCUSSION

Mrs. LLOYD. Thank you very much, Dr. Parker.

I'd like the panel to give the Committee additional insight on Dr. Overskei's and Dr. Parker's recommendation that the \$5 million be used for design and that we don't move ahead with the construction this year.

Lee, would you like to give us your insight?

Dr. BERRY. I think that—excuse me—the question, excuse me for ignoring the proprieties of the bureaucracy. I believe that the focus within the construction money plan is, in fact, on developing an optimum design. So, while the formalities of beginning construction are important, I think the question is, technically, what is the correct thing to do? And I believe the focus of the work that's proposed by Dr. Furth is essentially the same that Dr. Parker and Dr. Overskei is indicating.

Mrs. LLOYD. So there's no disagreement in this panel?

Dr. BERRY. Well, I'd be presumptuous on my part to—I think the important task in the near term, since construction cannot be started at a serious level with the funds that were indicated of a year ago, then the important task is to optimize the design.

Mrs. LLOYD. I guess this member wonders what difference 1 year can make, and also I question what \$5 million could do in terms of construction money. So I guess my comments may be one and the same.

Dr. FURTH. Would you like me to comment on it? Yes, I think Dr. Berry has said it very well. This \$5 million is not being used to dig any holes in the ground or manufacture any pieces, it is part of the ongoing design effort. And the reason why we value it in the form of PACE is because it sends a signal to the world, a small signal but still a finite signal, along the lines that you mentioned. That this project is still proceeding. And that is a very essential feature if we are going to get the rest of the world to take us seriously.

Now, on the topic of improving the design, I don't think that things are quite as uncertain as possibly might have been concluded. It is true that in the course of the year a kind of very nice breakthrough has been made in coil design, but that doesn't change the envelope of the machine. It doesn't change the plasma physics part. It will introduce a delay of months in ideas if we decide to incorporate it. And all in all, it is much less of a threat to getting this machine built than are these various financial rearrangements and reductions. That is the real threat in getting—

Mrs. LLOYD. Is this a step backwards or a step forward?

Dr. FURTH. The new design is a very large step forward because it is a very clever way of getting higher fields and higher currents for the same price as before. So, if as we look into all of the ramifications we can confirm that this can really be done, we will certainly adopt it. But that by no means puts the project in disarray. It just helps us do more with the available funding.

And similarly on the topic of keeping our eyes open and our project open for additional heating. The machine is equipped to accept additional heating. Nothing needs to be changed other than the planning for the development of electron cyclotron heating and so forth.

Mrs. LLOYD. One of the focus, the areas that we focused in with Department of Energy was the attraction of other nations to participate in the CIT. Where do you envision we will be this time next year—

Dr. FURTH. Well, I think—

Mrs. LLOYD [continuing]. As far as participation?

Dr. FURTH. I think it is very sensitive to what we do. For instance, if we were to abandon the idea that CIT is a real construction project, we will be exactly nowhere. On the other hand, if we show some vigor in moving towards a real machine, we are already making very good progress with a number of the other parties and I think for the moment they are already disheartened, but I think they will come back in.

Mrs. LLOYD. Well, what are the technical risks that are involved in building the CIT?

Dr. FURTH. First, one has to be absolutely certain that the engineering is conservative, and I think we are making sufficient effort to ensure that and we are running an R&D program that is looking at all of these matters very carefully, and I don't think that we will fail on that account. I think that is being done very carefully.

The other matter, the predictability of plasma physics, I discussed in my previous testimony, and there there are often surprises, but quite a large part of the time these days the surprises are good. And so, if you take the point of view that you must be able to predict and understand everything before you move, you run a very large risk of losing out. In the history of fusion, that happened to us once before in the late '60s when a mood of intellectualism was sweeping the Nation and there were guilty people such as myself who very much urged this approach of understanding all of the physics before you do anything. And then along came the Russians who hadn't heard about this and they gave us the tokamak. They didn't fully understand what they were doing. They just had a good intuition, and they were just moved miles ahead of us. And that is historically how progress has been made in fusion.

Mrs. LLOYD. Thank you, Dr. Furth. Any others of you like to make comments at this time?

Dr. Overskei?

Dr. OVERSKEI. Well, I'd like to indicate that I think that one of the reasons why we wanted to—why we proposed that the \$5 million construction be deferred and directed into the design is that we have some theoretical models that have not been fully developed and we still need some more time to develop them, to test our existing experimental data base. It's true we have an enormous data base, and it's also true that the basic envelope of the machine which is—will probably not change very much. But nonetheless, an additional year of theoretical understanding and corroboration and testing those theoretical models with our data base may likely change the ability of the device, or our ability to predict as to what the device's performance is and associated with that the consequential cost. Because even though the envelope may change, certain things that you put into that envelope or add into the facility to attain your objectives are likely to change.

Secondly, and probably the thing that concerns me most is whether or not, or what step one needs to take to ensure interna-

tional cooperation or participation. One would like to be able to leverage your program with other people's money if at all possible; in other words, foreign participation funds. I would propose that a \$5 million construction doesn't sound like a real big put on the part of the U.S. Government if I were in the European or Japanese community. If you really want to get some participation, then the program and the U.S. Government is going to have to be willing to put more than a \$5 million put, but more like a \$50 or \$50 plus million put to show a commitment.

Similarly, that commitment means either that you authorize and appropriate the funds now or you obtain those funds out of existing programs. Now both of those are alternatives which are within the prerogative of the Congress and the Department of Energy. But a strictly \$5 million "contribution" now will not solve the qualms or obtain—of the international partners, nor will it obtain their commitment and their recognition of our "resolve" to do a CIT or a burning plasma physics experiment.

There is another aspect associated with this, which is that in 1990 it is a time frame where the ITER program, the first phase of ITER comes up to conclusion, at which point all of the parties are supposed to assess what it is that the ITER would be and they can decide whether or not they would proceed with the next phase. I would put forth, and I may be singular in this opinion but I shall put it forth nonetheless, that that would be an appropriate time scale where the U.S. would say that possibly its major contribution to an ITER is the construction and operation of a CIT because the viability, scientific viability of an ITER is dependent upon one's ability to obtain and demonstrate burning plasma physics, and associated with that you may also get at that time a significant contribution and participation from your international partners.

Mrs. LLOYD. Thank you very much.

Dr. Parker?

Dr. PARKER. Well, I would just like to emphasize that my concern is not that the \$5 million would be used for digging a hole in the ground. I'm quite sure it would not. And in some sense, as far as the design is concerned, the money would be used in approximately the same way for design or construction.

My concern, my major concern, as I indicated, is the lack of a thought-through plan that shows how this device will be built and the effect in the out-years on the remainder of the fusion program. I don't, I have not seen that and I think that many of my colleagues are quite concerned that we don't understand the ramifications. So I believe we need this time to understand better the constraints that we're dealing with and to press ahead in a more collective sense.

Mrs. LLOYD. Well, what is the status of the organization and management plan for CIT, Dr. Parker?

Dr. PARKER. The organization is a, in fact, has national characteristics. In fact, my institution, MIT, is heavily involved in collaborating with Princeton Plasma Physics Lab as are some of the other laboratories such as the Oak Ridge National Laboratory. So that's the organization.

On the other hand—or at least the project structure. On the other hand, the project is managed and run by the Princeton

Plasma Physics Laboratory for the Department of Energy, so my part of my comment has to do with the representation of the other players in the management structure.

Mrs. LLOYD. Well, who will be accountable to see CIT completed?

Dr. PARKER. I think that's a very pertinent question, and it certainly is true that there has to be an identified institution to do that. I think that would be part of the details to be worked out and to putting into place the structure.

Mrs. LLOYD. You know, we did have problems with TFTR. That the organization was so complex it was rather detrimental.

Dr. PARKER. I'm not familiar in detail with those problems, but I do appreciate the difficulties. I'm not trying to, certainly, underestimate the difficulties of carrying out a project by a consortium-type of approach. There certainly are problems there. We have examples of great success in this area; in the fusion program, for example, at JET, which is, in effect, a consortium of European laboratories.

Mrs. LLOYD. Well, that is true, but isn't there always a lead agency?

Dr. PARKER. Well, the agency, in this case, of course, the Department of Energy, is ultimately responsible for disposition of the funds, and the agency could be established just to guide the development of the project, as in the case of JET.

Mrs. LLOYD. Well, I feel there has to be one, an individual or group under the Department of Energy to see that the project is successfully completed.

Dr. PARKER. Madam Chair, you may be quite right, and that may, in fact, turn out to be one of the laboratories, or, as it is now, the Princeton Laboratory.

Mrs. LLOYD. But it has not been designated?

Dr. PARKER. It is presently being designated as Princeton. But my real point I'm bringing up is this representation on the part of the other participants, since it is a major fraction of and will consume a major fraction of resources in the 1990s, I believe other laboratories should be represented as well.

Mrs. LLOYD. Thank you. Mr. Morrison?

Mr. MORRISON. Are we in a bind because we need to pour cement to demonstrate to our partners around the world that we're really serious about this? And yet I hear you sort of along the table saying, "Well, we're just going to design this thing and we're not going to dash ahead with the CIT. That won't be the next generation of machine that it needs to be." Am I mishearing you?

Do we have to do more than just the \$5 million for design? Dr. Furth?

Dr. FURTH. I think it would be a splendid thing to do a little more; yes. I think I don't share the view that taking the \$5 million and making it zero will restore the confidence of the world in the continuity and purposefulness of our actions. But to raise it to some more sensible sum like \$20 million I think would make an extraordinarily favorable impression. Thank you for asking.

Mr. MORRISON. Sensible sum. Okay.

Are we in a race with our international competitors/cooperators for ignition?

Dr. FURTH. Only modestly, I think. The Italian government seems to be rather keen to build something called the Ignitor, and you may remember Professor Coppi, who testified here last year—and you can look up this testimony—is the head of that program. And they seem to be quite keen on it. It's a somewhat smaller, more ambitiously engineered project. There are also germs of ignition ideas elsewhere, but I think the Italian one is the only one that is being taken rather seriously at a governmental level.

Mr. MORRISON. It's fair to say that we're ahead of the rest in the ignition area?

Dr. FURTH. I think that's true. I mean, in the design sense. Of course, if we can't build the designs, then that doesn't mean very much.

Mr. MORRISON. But they haven't achieved the 300 million degree?

Dr. FURTH. They have not. They are somewhat below. In terms of parameters we use for comparison, like how close you are to break-even, we're somewhat ahead, like a factor of 2. In terms of other parameters like how long is the energy confinement time, the JET people in Europe are ahead. So it isn't that we're just brilliantly leading the world. There's sort of a mix, and fortunately, we still are ahead in those things that we consider most important.

Mr. MORRISON. Those things that are most important, are we neglecting the box that's going to hold this reaction when it occurs, the engineering side of the house? We've been talking physics here.

Dr. FURTH. I think the simple answer to that is yes. We have focused the effort on the physics side, because I think we have felt rightly that we have to persuade our supporting agencies and the Congress that we really can get there with the plasma engine and that the physics' outlook is good enough so people will want to buy that engine. And so we have treated the engineering development as somewhat secondary, which may not be all that smart, because to some extent the kind of plasma engine you need will be affected or has been affected by what the engineers say the materials' problems are.

Mr. MORRISON. But that most likely will come along?

Dr. FURTH. With your help it may.

Mr. MORRISON. Yes. Um-hum.

I hate to ask for dates. I notice that the material that was—that Dr. Storm sent along had some dates, but, you know, that's really very helpful I think from our point of view. Because we're looking at advanced lightwater reactors filling a certain gap, we're looking at, then, some of the advanced reactors going beyond that. And is our date of 2030 really out of bounds?

Dr. FURTH. Well, if you're still asking me, then let me comment briefly.

Mr. MORRISON. I want to open it up——

Dr. FURTH. Yes.

Mr. MORRISON [continuing]. To the whole panel on this one.

Dr. FURTH. Good. Let me make a brief comment, then I'll shut off the microphone here.

I thought what Dr. Hunter said was rather to the point. The smarter we make the device, the easier and quicker will be the development path and the sooner we'll be there, and I feel that very much.

I would only add one other thing. If we could find some application for D-T reactions and 14 meV neutrons other than boiling water, then that might bring about a use sooner. But so far a convincing case has not been made.

Mr. MORRISON. I'd love to hear some predictions, gentlemen.

Dr. BERRY. Having written 2030 on a piece of paper in the back while you were discussing it, I certainly think that's a reasonable type of date to discuss commercial power. But I also think that in examining the question what we're trying to do is lay out this decision matrix as we're going to have information on advanced fission such as date and the CO₂ hits us 5 years earlier and the like, and fortunately nature just hasn't provided it to us.

Fusion is a qualitatively different type of option than advanced fission. We could lay out the development schedules for advanced fission now with some certainty. We can't do that with fusion.

Mr. MORRISON. Um-hum.

Dr. BERRY. And so I would rather think of the whole process in terms of the development of options in the face of ignorance as to what's going to happen, how bad CO₂ is going to be, when it's going to hit, how costly acid rain control is going to be. That rather we need to look at a strategy that develops options. And the goal, I think, of fusion should be to see is there anything in this rather adventurous undertaking that can give us an option sometime in the 20th century. Because I can argue that I need fusion in 2030, I can argue I need fusion in 2050, I can argue I don't need it till 2150.

What I do need is the information on which I can plan, on which I can know whether or not options are available. And so I think what we're looking at fusion is a level of funding and research which may lead to uncertain dates but allows us to get the information to knowing whether or not the option exists in the same sense that an advanced fission option does, in fact, exist now if we choose to exercise it.

Mr. MORRISON. Good. Others? Dr. Parker?

Dr. PARKER. Well, I agree also with Dr. Hunter's analysis earlier. Taking it a step at a time, I think that the ignition step would—a 10-year time frame is appropriate for that. And then beyond that I think the comment that how fast fusion can be developed will depend in part on how much we know and how much we learn I think is a very shrewd observation, and many of us, in fact, Dr. Okawa, from General Atomics, pointed out to many of us this past summer that one difficulty is the fact that as we now conceive of fusion it comes in a rather large size; namely, the size of a large fission power plant, 1,000 megawatts, something of that size or scale. And so, you know, even in the case of fission which is well developed we know how long it would take to build a plant of that size. If we said less than 10 years, we would be kidding ourselves.

So, to go beyond the ignition stage to commercialization, at least in this very large-scale fusion step, it would certainly be 30 years would be reasonable. Now, if we do make progress in understanding and in the technologies to reduce the size, to learn how to make 100 or 200 megawatt reactors, then it could be sooner than that.

Mr. MORRISON. Anyone want to argue with those positions? Dr. Overskei?

Dr. OVERSKEI. Well, I'd just like to say that the magnetic fusion program as it's presently outlined and mandated by the Department of Energy at present is to essentially obtain the information so that one can make a decision as to the merits of fusion with regards to energy production. And has already been eloquently pointed out, I think we will be able to—we have the technology today to build an ignition device. The question is a little bit more of the money and therefore that will constrain the size.

But the step beyond that gets to be a bit more difficult because we will also—I don't have any difficulty in envisioning within 20 to 25 years of a device that will actually produce energy. The real issue is whether or not that device produces energy economically and in such a manner that meets the sociological constraints that will be put on that system, and that is much more difficult to gauge.

So, if you are talking about when do you have something on a grid as 2030, I consider that to be somewhat problematical because we don't know. Although we have great ambitions for what we'll be able to do for you, it is difficult to predict.

Mr. MORRISON. Of course, it certainly helps our cause in getting you more money if you were the ultimate answer to the greenhouse effect and global warming.

Dr. OVERSKEI. But we may be.

Dr. BERRY. We think we are.

Mr. MORRISON. That's right. You may well—Dr. Linford?

Dr. LINFORD. Well, I would like to also agree with Dr. Hunter's assessment that the development path for fusion depends greatly on the size of the units that you have to study in order to get from here to commercial power. If those are extremely large, then it takes a much longer time, much larger investment, and the pull from the market side will be much less because of the higher risk and higher capital cost. If those steps are much smaller in terms of their capital cost, then the possibility of learning quickly from the steps to a commercial situation is much more, can be much more rapid. And the concept and improvement area that I talked about is very essential, in my view, in trying to come up with the kinds of inventions and advances that allow that kind of low cost small step to be taken.

And it's very important when you get to the point when you want to assess the potential of fusion as to whether you have the data available to you to know do you only have large systems available to you or is there the possibility for these smaller, low cost, lower capital cost steps.

Mr. MORRISON. I think this subcommittee understands that very well since we're in the nuclear fission arena working for the reduction of modular smaller units preapproved by the NRC that can be put into place rather rapidly, where utilities now won't dare place an order for anything because of the uncertainties. So, thank you very much.

Mrs. LLOYD. Thank you very much, Mr. Morrison. Mr. Schiff?

Mr. SCHIFF. Two things, gentlemen, please. One is I, frankly, got a little confused in the discussion of funding of the construction of

CIT. Specifically, Dr. Furth, are you saying something different than Dr. Parker and Dr. Overskei in terms of putting money into construction at this time?

Dr. FURTH. I think so, so let me repeat the record for you. We were slated to receive \$31 million for construction of CIT in 1990. And when I say we were slated, that was part of the 5-year plan which Mr. Morrison mentioned earlier. What happened was that this \$31 million was cut to 5½ million, and so we're having this rather esoteric discussion here whether the international confidence would be more restored by cutting it to zero or by putting it back up to something more sensible like \$20 million or leaving it alone.

Mr. SCHIFF. What about the argument that the extra 5 million, if I'm understanding you correctly, if put into further design research could ultimately produce a better CIT? That's what I understand the other witnesses to be saying.

Dr. FURTH. The money, in fact, is all being put into design—design and R&D. At the moment there is no manufacturing, no construction. It is purely an issue of how should this money be named. It is all being spent for the same purpose.

Mr. SCHIFF. Well, then there is not a difference between—

Dr. FURTH. There is—as I sense it, the issue is whether the present CIT construction project is to retain its identity as a construction project and go forward or whether we are to say let's start all over and give it zero, you see. So it's not a matter of how money is to be spent. It is more of a conceptual thing.

Mr. SCHIFF. All right. Second, I asked Dr. Hunter if he felt satisfied with the amount recommended in the Administration budget for the fusion research project, and he said yes. I listened quite carefully and I didn't hear yes from anyone else. Now maybe I missed something there.

But in terms of overall amount, the Administration proposes apparently \$349 million for the next fiscal year. I would just like to ask for, and I realize you could, perhaps, go on for quite some time on the whys, but I'm asking you to—respectfully I'm asking you to skip that. What would your overall figures be, if I may ask anyone on the panel who has a different figure than \$349 million?

Dr. FURTH. I think one number that immediately leaps to mind is the number that used to be in the 5-year plan which was created 1 year ago with agreement, as I understood it, of the Department of Energy and OMB, and that plan called for \$375 million.

Mr. SCHIFF. At this particular juncture, you mean?

Dr. FURTH. Yes.

Mr. SCHIFF. All right. Is there—just whatever.

Dr. LINFORD. Yes. As I said in my testimony that I believe that that 375 is needed in order to take advantage of the investments that we have now made. Part of the reprogramming action, in fact, was to increase the operating time of Doublet III and TEXT because they were underfunded. Unfortunately, it was taken away from other activities, so that you are simply making it worse one place and better someplace else, as opposed to actually being able to capitalize on the investments in the overall fusion program in a balanced way.

And, as I stated last year in my testimony, that we need an increment above that in order to really begin substantial construction on the CIT. So I think that those two things together give you a feeling for the quantity of money that would allow us, on the one hand, to move forward with the investments we've made, to attack the problems which face the fusion program, as well as to make a respectable start on the CIT construction.

Mr. SCHIFF. Any other budget comment? Dr. Overskei?

Dr. OVERSKEI. This comment is related to both of your questions. The underlying concern within the community associated with whether or not \$350 million is adequate depends upon what it is that you and the Department of Energy envisions the U.S. fusion program to be, Magnetic Fusion program. If you are going to do a significant restructuring, then the funds are probably adequate, although not optimum, and that is associated with how you manage and organize the CIT, and it's associated with what degree of probability or confidence you are willing to accept with regards to the CIT's design and ability to reach its design objectives, no performance objectives.

To optimize the existing programs which are in place and to realize the performance, the engineering requirements of a CIT so that you can therefore demonstrate the plasma performance would likely require a greater budget than is presently being allocated. And, even though the amounts that have been mentioned by some of the other people are relatively modest, like up to \$375 million, I believe that that is also a significant underestimate of that which is necessary. The reason being that a lot of the programs that you presently see in the U.S. Magnetic Fusion program have already been significantly altered or pared down.

I hate to bring it up, but the expense—well, I will bring it up, and it is not to be parochial, but it is worth noting that some 3 years ago our program at our facility was at a level of about \$35 million 3 years ago, and then it was cut down to about \$27 million. So, if you took that \$35 million and escalated, then we should be up at about 40 some million. This has occurred at numerous other facilities. We are also only operating half time, and, as I said, we are the second largest facility in the U.S.

So that there are ways in which you could spend the additional monies in optimizing and utilizing to the full degree the facilities, the resources which you have already developed and paid for in the U.S. fusion program. So the issue about the \$5 million is to design and get in place what we consider—this is from my perspective; I still feel that I disagree with Dr. Furth somewhat—is to put in place a structure that we feel is optimum from the program point of view, a design that we feel will have the highest probability of reaching success, and the commitment—and a physics model, theoretical model to corroborate our existing data base, and then a commitment on the part of the fusion program, the Department of Energy, and Congress to carry this through, so that we can demonstrate to you an ignited or a burning plasma and proceed on with the next step which is so urgent and, as Mr. Morrison has pointed out, something that is really necessary. The next step being to produce electricity in some useful fashion.

Mr. SCHIFF. Anyone else want to choose to comment, just on the overall budget figure?

[No response.]

Mr. SCHIFF. Okay. Thank you, Madam Chair.

Mrs. LLOYD. Thank you very much, Mr. Schiff.

I'm delighted that Mr. Walker, the highest ranking Republican on the full Science and Space Subcommittee is with us this afternoon.

Thank you for being here, and we'd like to recognize you for any questions or comments you may have.

Mr. WALKER. Thank you, Madam Chairman.

I'm just interested in a general comment on the technology. Is there anyone doing work, any of your labs or do you know of work going on using helium III instead of the tritium cycle?

Dr. FURTH. I think many of us use, every once in a while, a little helium III mixed in with the deuterium. Any tokamak can do this, and at the present level nothing very radically different, of course, happens. I think the people who are pushing this fine art at the moment the farthest are the JET people, in England, who are accelerating helium III ions with radio frequency waves and making a lot of reactions. So all of us have the D-helium III in mind. All of us can do it. But, basically, it doesn't—being interested in the ultimate target of a D-helium III reactor doesn't change the plasma physics that you do today.

Mr. WALKER. Are there advantages or disadvantages in using helium III over—

Dr. FURTH. Well, the great advantage is that you would get fewer neutrons and more charged particles, thus further reducing any problems about waste disposal, possibly bringing in potential for direct conversion. It makes a very nice reactor.

The two drawbacks are, firstly, it's a little harder to do in terms of you have to run at still higher temperature and better confinement. I think we could overcome that. And, secondly, where is the helium III? That was a terrible block for a long time, until recently when it was discovered there's quite a lot of helium III on the moon, and NASA has gotten quite interested in looking into that. So that has generally stirred up all our interests in this possibility and many of us are quite keen on thinking about that.

Mr. WALKER. So until recently, you're saying, that one of the reasons for not pursuing this more aggressively was the scarcity of the resource and now we do have reason to believe that there—

Dr. FURTH. Yes.

Mr. WALKER [continuing]. May be a fairly substantive amount of the resource available, so that will ignite some interest in—

Dr. FURTH. Yes.

Mr. WALKER [continuing]. Moving toward that kind of a cycle.

Dr. FURTH. Already has. Yes, sir.

Dr. PARKER. I wonder if I could comment also on that. In addition to the source of helium III there is the question of more stringent requirements on the science, on the plasma physics, and an additional thrust of the, the emphasis on studying and understanding better the confinement is that if that were accomplished then it would become much more feasible to take a reactor using helium III more seriously, and for that matter a reactor using only deuteri-

um, which would avoid the problem, if you call it that, of breeding tritium from the lithium blanket. So this is really in my way of thinking not just a question of the source, but—and not just a question of using a smidgen in our existing experiments, but really one of fundamentally improving our understanding and ability to control the confinement.

And one last point on this is that——

Mr. WALKER. Let me just follow up, though.

Dr. PARKER. Yes, sir.

Mr. WALKER. Do I understand correctly that what you're saying then is that the use of helium III might make a commercial-type reactor more practical if, in fact, you could achieve the——

Dr. PARKER. If—what I'm trying to say is that the physics conditions or science conditions for using helium III are more stringent than for using tritium.

Mr. WALKER. Okay.

Dr. PARKER. The deuterium-tritium is the easiest thing, and then deuterium to tritium, or deuterium-helium III is the next most difficult. What I'm trying to say is that the new emphasis in the program on the transport study would have, in fact, as one of its output potential for taking the helium III fuel cycle much more seriously.

Mr. WALKER. But, if we were able to use helium III in the reactor, and you do not get the contamination problems that you do with the other cycles, doesn't that improve its commercial ability and doesn't that speak to the problem that I think someone spoke to a minute ago, and that is some of the sociological and environmental problems with bringing such a reactor on line?

Dr. PARKER. That's certainly true. With regard to producing activation, the great benefit of helium III is that the D-helium III reaction itself produces no neutron, actually just a proton, and so it produces no, has no radioactivity associated with it whatsoever. Now that statement implies that one has a highly helium III rich reactor and a small, relatively small amount of deuterium because deuterium itself will produce a few neutrons. But the sense of your overall question is correct; namely, the D-helium III cycle is very advantageous regarding environmental impact.

Mr. WALKER. And the budget that we are contemplating at the present time will include some continued research and a continued look at using a helium III cycle?

Dr. PARKER. There is some work being supported at the University of Wisconsin specifically on this concept, and I'm not sure of the scope, how much is supported there. But there is work being——

Mr. WALKER. Are they doing anything to cooperate with the British that seem to be, or are fairly far along, if I understood Dr. Furth correctly?

Dr. FURTH. Let me have another crack at it. Getting ready for burning D-helium III doesn't really require to do, use as your plasma D-helium III. As Dr. Parker was saying, all it requires is to have an even better confinement scheme than for deuterium, but you can do it with deuterium just as well. The fact that the—in the sense the fact the British are using D-helium III is incidental.

What the money—the only money that is being spent specifically for D-helium III these days is for reactor studies. How would you

use it? And it is my impression that while Dr. Kosinski at Wisconsin is taking the lead on this I wouldn't be surprised if he is better supported from NASA than from DOE on that, but I don't know for sure.

Mr. WALKER. Yes?

Dr. BERRY. If I might make one comment. It would be an interesting horse race to see whether or not a D-T fusion reactor with the improved physics that would be required for D-helium III would economically work out to be better or not. So, it's clear that the D-helium III has more attractive characteristics in one dimension. However, because the physics is so much harder and we would have to use higher field magnets, have better materials available for the magnet structures, that it might be an economic horse race. Again, it's like the question of comparing advanced fission with fission. It will come down to an economic issue and the specific technologies that you have available.

Fortunately, as Dr. Furth said, the plasma physics route, which we spend the bulk of our money, is the same direction.

Mr. WALKER. For both?

Dr. BERRY. For both approaches. And the key thing is having some modest level of reactor studies under way to understand what the potential is and paying attention to the technology development such as the high field magnets and structural materials which would also be required. However, these would also be used for D-T. So I don't think we really have any choices to make with respect to choosing fuel cycles, rather of being aware of the potentials in both dimensions and making sure we're not foreclosing anything because of the R&D choices we make.

Mr. WALKER. And because the plasma physics is essentially the same, then the time lines that we're talking about earlier year would be approximately the same for both?

Dr. BERRY. The D-helium III would most likely follow. Well, it depends on whether you believe the plasma physics will be governing on the time scale or the technology will be governing. If the plasma physics is governing, D-helium III would follow D-T. If technology winds up being the governing part of the step, which it could well be, depending on how rapidly we support the long range efforts, then they could well be on the same time scale, but they would both be on a much later time scale.

Mr. WALKER. And that speaks nothing to supply of the helium III? In other words, even if you had vastly more helium III available to you right now, that would not change that time line?

Dr. BERRY. Right. And, in fact, there are fuel cycles, the catalyzed D-D cycle, which are quantitatively better with respect to neutrons than is D-T but yet not quite so good as D-helium III, which represents another intermediate step.

Dr. PARKER. If I could comment, though. As you can tell, when you get 5 scientists together they find ways of quibbling with each other.

In regard to if, your comment on whether, if helium III were more abundantly available would we be pursuing that with more vigor, my gut feeling is in the affirmative because of its qualitative advantages over both deuterium-tritium and deuterium-deuterium fuel cycles.

Mr. WALKER. Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Walker.

Thank you, gentlemen, for being with us, and we look forward to hearing from you again later in the year.

Our last panel will include Dr. Erik Storm, from Lawrence Livermore Laboratory. He'll talk about a different branch of fusion known as inertial confinement. And it's always good to see Dr. Stephen Dean back with us, from Fusion Power Associates, who represent American industry for fusion energy development.

We're glad to have all of you with us. And you may proceed, Dr. Dean.

PANEL 2

STATEMENT OF DR. STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES, GAITHERSBURG, MD

Dr. DEAN. Thank you, Madam Chairman. I thought I was going to have the pleasure of following my good friend Erik Storm. As you know, I think this is Erik's first appearance before this committee, and I'm very happy to see—

Mrs. LLOYD. You know, we usually go in alphabetical order.

Dr. DEAN. I see.

Mrs. LLOYD. So I hope you will forgive us today.

Dr. DEAN. I'm very pleased to see this committee taking a more active interest in inertial fusion as represented by the fact that Dr. Storm is here to testify. As you know, I personally feel that what we're all trying to do here is to develop fusion. We don't really care if it's magnetic fusion or inertial fusion or some other technology for fusion. What we want is to develop maybe one or more technologies which will bring fusion to the marketplace.

There's been several references here in the previous speakers to the fact that we had a plan in 1976 that led into the Fusion Act that said the fusion budget would have to double and we would have to build some engineering test reactors, and if we did that, then for like \$20 billion we could have power on the line in 20 years. I happen to have been the chairman of a national task force that developed that plan in 1976. We had a range of end-points for fusion that ranged from 15 years to 30 years depending upon the profiles of the budget commitment.

I'd like to reiterate the fact that the time scale on which you can have fusion depends directly on our budgetary commitment. It depends not only on how much money, but it depends on what you're willing to spend that money on. If you spend \$1 billion a year and you spend it all on physics, all on theory, computation, and running all of the existing machines round the clock, if you did that for 20 years you would not have fusion. You must spend the money on some new things in addition to doing the old things well and developing the physics.

You must get into the engineering. You must get into technology development. You must get into reactor design. You must build engineering test reactors. You must build devices that make fusion power, so that you are forced to handle the neutrons and all of the rest of the technologies that goes with making a power system reliable. So you must be willing to make those commitments. And

until and unless the country and the Government and the DOE decides that they are going to start building some new facilities of this type, you will never have fusion power.

The Fusion Act of 1980 called for an engineering test reactor to be built during the past decade. That reactor has not been built. The largest machine we have today is the TFTR. It was in the fiscal 1976 budget. You know, we're now talking the fiscal 1990 budget. That's 14 years ago we put our last major construction project in the budget.

All of the progress that's been achieved as shown in the graph that's on the second page of my testimony, which shows TFTR at the top of the pile, all of that progress has been accomplished in facilities that were built during the 1970s, including the TFTR, which was commissioned in 1976 budget cycle.

So here we are, you know, 14 years later and we're talking about when are we going to have fusion power, and the answer is still the same. If you want fusion power, you have got to start making commitments again to construction. In the U.S. that means at the moment the CIT. Now CIT is not the most ambitious thing that we could think of doing. If you all remember from several years back, we had some machines that we were proposing to build as follow-ons to TFTR which were more ambitious than the CIT. They were machines that were going to burn for 100 seconds, and not, you know, 10 seconds. We had at times past proposed to build engineering test reactors in this country. The reason that we are proposing the CIT in its present form is not because it's the most ambitious thing we think we know how to do. It's the thing that we've wound up with trying to make it cheaper every year to try to find something that the Government would pay for. And so the CIT is, in fact, kind of our attempt to find something that's cheap enough to do that will get us a little bit more progress in the 1990s than we're able to do in the TFTR. So, yes, we would like to go forward with the CIT. But, you know, if the country really was serious about developing fusion power, we would like to go forward with something more ambitious than that.

We've lost, as several speakers have mentioned, about half the fusion budget during the '80s.

We have got to get back on the track of getting that budget growing again, and we have to add to that construction money. You know, not \$5 million. I mean, that it's sort of absurd for us to be sitting here arguing about \$5 million for CIT. CIT is a \$400 million project. We could propose to you a billion dollar project that would make sense.

And \$5 million doesn't make the difference whether that project is going to be built or it isn't going to be built. It's nothing. It's not a down-payment on a multi-hundred-million dollar construction project. That's why people can't tell you here whether it should be construction money or operating money. It doesn't make any difference to the project whether it is construction money or operating money. It's not enough money for the project to be taken seriously.

People have asked when, what would it take to bring the Europeans or the Japanese into that project. Well, it's going to take more than \$5 million on the part of the U.S. Government for anybody wanting to buy into that project. You know, it's going to take \$50

or \$100 million coming down the pike if you really think that other people are going to buy into that project.

So I think the important point here is that the fusion program has made a lot of progress based on facilities that were built 10 and 15 years ago. It can make more progress by operating those facilities for a number of more years. There's a possibility that by making a strong effort on transport that we will understand things a little better a couple of years from now. There's also a possibility that a couple of years from now we won't understand it any more than we do now. So at some point you have to bite the bullet and go on and build some new facilities based on whatever physics you've learned and whatever your best judgment of your community is. At the moment that facility is CIT at a minimal price, or it could be something more ambitious if there were more money available.

My own testimony, I've asked that the \$13 million that was withdrawn from some present programs in the reprogramming for this year be restored. I come up with that number as just as an interesting number that I think would make sense, but I could just as equally argue for \$50 or \$100 million depending upon how much money you think that the Congress might be willing to restore to the budget. Certainly the program could spend \$100 million more a year profitably if that kind of money were available.

You know, I think the reprogramming action on the part of the DOE makes some scientific sense. The program is ready to make a concentrated effort at understanding physics. But the monies that that—the programs that that monies were taken out of, that \$13 million, also made sense. Those were good programs.

The ATF program at Oak Ridge, a brand new stellarator, it's a darn good experiment. It's going to tell us something important about the physics of tokamaks and the physics of alternate concepts and how to make a better reactor, and that machine is ready to roll. Cutting that program back just as it's starting to come into operation doesn't make any sense whatever. With the kind of money that's being saved, it doesn't make any sense at all to me.

Same way with the Los Alamos machine. You know, it's a good machine. Half the money's been spent. You ought to get it built. You ought to get it built and operated. Cutting the few million dollars out of the program and just making everything more difficult on programs like that is not helping to get fusion physics understood.

On TFTR we've now postponed the tritium operation of the machine. You know, that was the purpose on which the machine—for which the machine was built in the first place. I would like to see a lot of effort being put into making TFTR perform a little bit better so we got break-even conditions in D-D, and I think Princeton is committed to doing that. And, if we can do that, I think we ought to put the tritium in the machine. We ought to be paying the money now to get ready to do that, not waiting to see several years down the road, you know, whether or not we want to come back to it.

So I think we ought to put that money back in the budget, and I think we ought to get on with those programs.

Finally, I would like to make a comment, just before Erik talks, about the fact that—it hasn't been mentioned yet today, that the DOE has put some money in their budget, in the magnetic fusion budget to do some system studies for inertial confinement. I think that's a good idea. I know there's some resistance to spending that kind of money within the magnetic fusion budget on inertial confinement because they figure that's not their business, let the weapons people who support that program spend that money. But, as I have mentioned several times in the past years when I've testified here, the weapons part of DOE has not been spending adequate money on the potential civilian applications of inertial fusion. Their own budget has been held back for that program over the years, and they don't have the money to spend on both the system studies, the reactor studies, and the technology work for the kinds of drivers that you need in inertial fusion for civilian applications.

So I welcome the fact that Dr. Hunter wants to spend some money on inertial fusion. I would like to see those activities continue and not just be a one-shot thing. As I have said in past testimony, I think the system studies program even for magnetic fusion has declined below the critical point in recent years. We have hardly any reactor studies work anymore. We have hardly any work on things like D/helium-3 reactors or alternate applications of fusion. So I think we need to have a strong systems analysis program that incorporates the best ideas we can think of for both magnetic and inertial fusion, and, if Dr. Hunter is willing to put money in that, I strongly support it.

Finally, I note as I have in past years that there is another element of the inertial fusion program, the heavy-ion fusion accelerator program, which Dr. Hunter is responsible for, and it is a part of his basic energy sciences program. That's a potentially very important driver for inertial fusion civilian applications in the future. That program has made good progress, and it should have a new accelerator, a \$27 million accelerator at Berkeley which has never survived the DOE budget process. And I encourage the Committee to take an interest in that program also.

Thank you.

[The prepared statement of Mr. Dean follows:]



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STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY
FEBRUARY 21, 1989

In 1980 it was estimated that the fusion budget would have to be doubled, and a commitment to building a tokamak engineering test reactor would have to be made, in order that fusion power be demonstrated by the year 2000. Since 1980 the fusion budget has declined to about one-half of its 1980 level and no new tokamak construction projects have been initiated. As long as this situation continues, and until it is corrected, fusion power will not become a reality.

Of course, fusion is not alone among the many new and improved energy technologies that have been neglected during the 1980's. Our national energy policy has been to increase our dependence on fossil fuels and to simply not respond to the many objective studies showing an impending energy shortage. "Let the private sector worry about it," has been our de-facto public energy policy. The electric utilities have given us the private sector's response to this challenge. They have made essentially no investment in new installed base-load capacity during the past decade.

Fortunately the American public's awareness of the global environmental damage that may result from continued burning of fossil fuels is on the rise. Hopefully this public awareness will result eventually in a change in government policy towards the funding of energy technology development, including the development of a fusion energy source. It will take about \$20 billion over about 20 years to develop and qualify any new energy technology for commercial central station power. Until and unless the government recognizes that this level of investment is good and necessary, the nation's energy future will remain insecure.

In spite of the declining budgets of the 1980's, the fusion program has made steady progress on achieving the conditions required for fusion power, by using facilities that were committed in the 1970's. The accompanying figure shows the key measure of this progress: the product of temperature, density and plasma energy confinement time ($Tn\tau$). The TFTR at Princeton, for example, has produced plasmas with temperatures of 300 million degrees Celsius. Such temperatures were almost incomprehensible to imagine ten years ago. In so-doing,

AND TO IMITATE THE SUN"

"TO TAKE FROM THE SEA

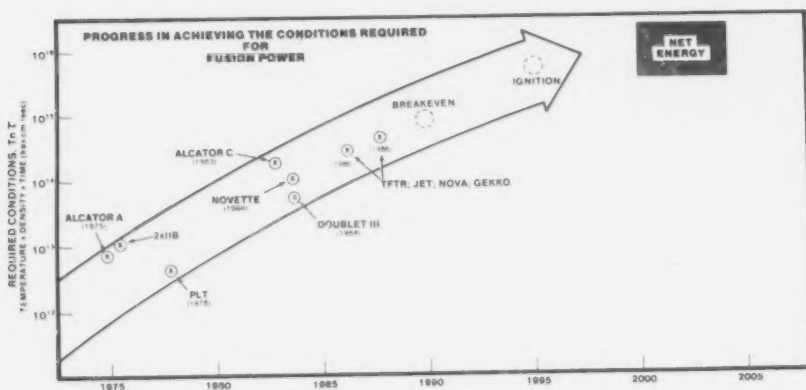
TFTR has met or exceeded its original minimum design objectives of producing a $Tn\tau$ product of $2 \times 10^{14} \text{ cm}^{-3} \text{ sec}$ and is very close to the regime we call "scientific breakeven."

The DOE FY 1990 magnetic fusion budget request is a big disappointment. Only last year the DOE and OMB sent to the Congress a five-year budget plan that included \$375M for magnetic fusion in FY 1990. Even at that level, the construction schedule for the Compact Ignition Tokamak was undesirably stretched out.

The DOE has asked for a "reprogramming action" in FY 1989 that would reduce activity on the ATF program at Oak Ridge, the CPRF program at LANL, preparation for DT operation on TFTR at Princeton and numerous smaller cuts at several sites. I would ask that these funds, totalling about \$13M be restored in FY 1990 as an add-on to the DOE's budget request.

I support the proposed program of systems studies aimed at assessing the civilian potential of inertial confinement fusion. I do not believe that the studies should be limited to gas lasers and heavy ion beam technologies, but rather should look at a broad range of commercially-viable driver technologies, including light ion beams and other types of advanced lasers. Also, I believe that these studies should not end in FY 1990 but be an on-going effort every year and should include a broader evaluation of the many possible types of magnetic fusion concepts also.

Finally, I urge you to continue to provide strong support to the heavy ion fusion accelerator activity which is managed at DOE as part of its Basic Energy Sciences program. That program needs to proceed with a modest accelerator project called ILSE which has not yet survived the DOE budget making process.



Mrs. LLOYD. Thank you, Dr. Dean.
Dr. Storm?

**STATEMENT OF DR. ERIK STORM, PROGRAM LEADER, INERTIAL
CONFINEMENT FUSION, LAWRENCE LIVERMORE LABORATO-
RY, LIVERMORE, CA**

Dr. STORM. Thank you, Madam Chairman and members of the committee. I'd like to express my appreciation for the opportunity to offer my opinions on the problems of fusion and particularly inertial confinement fusion, or ICF as we call it for short.

Now, I responded to your request and provided fairly long testimony for the written record. I'd like to have that entered.

Mrs. LLOYD. Without objection, your entire statement will be made a part of the record.

Dr. STORM. Thank you. I'd like to try to summarize in the next 15 minutes.

Recently there's been increasing interest and activity in the ICF research and development worldwide motivated by the long-term civilian energy applications as well as in some cases near-term military and defense missions. And it is, of course, because of these important near-term applications of ICF physics and technology for defense that ICF has been nearly totally funded out of the nuclear defense program activities within the DOE these last 17 years. The fact that this is, of course, an unclassified hearing means I can't go into great detail about that, but I'll try to summarize some of the activities later on in my comments.

I'm very pleased to report that the U.S. ICF program has made significant technical progress since it was last reviewed for you 2 years ago, and that the United States continues to lead the world in ICF research and development. Experimental results obtained during this period have put to rest some basic questions about the scientific feasibility of achieving ignition and high-energy gain for ICF, a requirement for both civilian and defense application of this technology. These results have motivated the DOE defense programs to begin planning for the construction of a Laboratory Microfusion Facility, or LMF as I'll use the short term several times during my comments.

In order to take full advantage of these results and this facility when completed towards the ultimate civilian goal of electric power production, I feel that the time is right now to initiate a civilian increment to the ICF program to fund research and development on energy technologies. Now, I'm in a difficult position here, but let me try to make it absolutely clear that I'm now talking about—I am talking about an increment to the diffusion energy effort, not a substitution of current or future funds for magnetic fusion energy research. Today, I'll briefly summarize the technical progress that justifies the recommendation I just made and I'll also try to comment briefly on the current state of ICF research in the international community and how the U.S. can continue to keep its current leadership position.

Let me just first briefly talk about the "why" of fusion and the "what" and "how" of ICF. As many people have said before me, it is my opinion also that the development of abundant environmen-

tally attractive energy sources must take a higher priority both on the national and international agenda. I also feel that fusion should be a major partner in that long-term energy strategy.

Now fusion, of course, clearly works. The stars in the night sky are examples of Mother Nature's fusion power plants, and that's very encouraging. However, it's clearly a long way from those to practical, environmentally attractive, and economically competitive earthly fusion power plants. You've heard the previous speakers talk about achieving temperatures of tens of millions of degrees, and that is, in fact, required to produce the fusion reaction. But we also have to confine the fusion fuel, or the plasma, long enough so it has a chance to produce energy, and therein lies the main difference between the magnetic and the inertial confinement fusion process.

In ICF the approach is to get not only the 100 million degrees, but also to compress the fuel to about 20 times the density of lead. Now such conditions truly exist only 2 other places in the universe: in the hydrogen or thermonuclear bomb and in interior of stars. The fact that ICF depends upon duplicating these conditions also explains why this technology is pursued for both defense and energy applications. Let me briefly discuss the near-term defense applications.

First, let me make it also clear that even when we achieve the ultimate objective of ICF fusion yields in the range of a few hundred to several—to about 1,000 megajoules in the laboratory we are not talking about either using ICF for fusion weapons nor testing or developing either nuclear fission or thermonuclear weapons in the laboratory. What we are talking about is duplicating on a micro or, perhaps, miniature scale some of the conditions that exist in the final thermonuclear state of the weapon or with specially designed experiments placed next to an ICF target some of the neutron and X-ray effects produced by a thermonuclear device.

Now, the availability of a microfusion capability then would significantly augment ongoing physics research and development efforts associated with our nuclear weapons design program. It is important to note that such R&D efforts have already begun to pay off at the Lawrence Livermore Laboratory with the Nova laser. The main payoff, at least in my opinion, however, in the national security sense would come with the potential comprehensive test ban, or CTB. In this case a laboratory microfusion capability would be an essential part of our ability to retain for a while at the national laboratories the personnel that constitute our nuclear defense expertise and without whom we would not support a viable nuclear deterrent. In a sense this would be a part of a national insurance policy until we were sure that the other side would stay with the CTB.

Now the defense applications I call near term because they require only modest extension of today's technologies and capabilities. In contrast, the primary long-term or civilian goal, a pure fusion energy source, requires significantly more technology development.

Now before I talk about ICF as an option for electric power, we have to address the question of scientific feasibility. How do you talk about truly recreating stellar conditions in the laboratory? Let

me first discuss briefly how we obtain the roughly 100 million degrees temperatures.

With a laser such as Nova, we can, in fact, focus about 100 trillion watts of laser light onto a ICF target about the size of 2 or 3 human hairs, and with that it is relatively easy to generate the intensities required to generate 100 million degrees. If you want, it is a high tech version of what you did as a kid. You took a magnifying glass and focused sunlight on a piece of paper. A very high technology version of that. And at hearings for this committee 2 years ago we showed you a photograph of a miniature star that, in fact, was heated up to the 100 million degrees.

However, this alone is not enough. We also have to confine D-T (deuterium-tritium) at high density and that requires something quite complex. Compressing this ICF target more than 30,000-fold in volume. To give you an analog, that's equivalent to compressing a basketball down to about the size of a marble. A formidable task. However, in the last 2 years, using an older facility we have demonstrated that we can in fact achieve such volume compressions. I had planned to show you a Viewgraph, but the frontispiece cover on my testimony shows a color rendition of neutrons actually imaged from a core of a compressed ICF target that is compressed to more than 32—nearly 32,000-fold in volume. It may not be very exciting to some of you, but for us in the ICF community it is as if 15 years' worth of preparation and R&D finally paid off.

Now this result was obtained, as I said, with Nova, which is the principal ICF experimental facility in the United States and in the world as well. The objective of the program there on Nova is to demonstrate that with properly scaled targets that are roughly about one-tenth the size of a full-scale, high-gain target we can achieve fusion conditions that not only test our understanding of the basic underlying physics, but also scale to conditions of high gain when driven with a 5 to 10 megajoule or 1,000 terawatt laser. Parenthetically, that is a laser with about 20 times the power of the Nova laser.

In the experiment, in my testimony, we achieved fusion conditions that you heard about before in the pilot of NTTau and temperature, the confinement temperature product. There also were a factor of only 10 away from those required for ignition. The same value as that obtained for the magnetic fusion community, and we're quite excited about that.

For us, perhaps more importantly is that these laser targets performance and the implosion results agreed very closely with theoretical predictions. It is a predictive capability. Over the next several years with the recent improvements just completed in the Nova facility and improvements we expect to complete in the next few years we will refine target designs and expect improvements in our results to fusion temperature and confinement conditions that will be within about a factor of 3 or less away from ignition.

Now all these targets were of the type called indirectly radiation driven targets, classified targets. In this concept energy from a laser is converted to radiation that is used to implode and heat the ICF capsule. The ability to study ICF in the laboratory has been limited by the energy and power available with present-day lasers. In the classified Halite/Centurion program, we use a portion of the

energy from a nuclear device in underground tests to implode ICF capsules and thereby extend the range of inertial fusion research. These experiments have allowed demonstrations of excellent performance, and together with the Nova results have put to rest, as I said, some fundamental questions about the basic feasibility of achieving high gain ICF.

Now let me briefly turn to the next natural phase of the ICF program, the Laboratory Microfusion Facility, or the LMF. The issues associated with building an LMF driver are a balance of cost and perceived time urgency for the country. To respond to this we at Livermore are pursuing two options that I call a time-driven, low-risk option based upon extensions of the Nova or laser technology and a cost-driven, higher risk option. Now the optical and industrial technology base is already significantly advanced since the time Nova was completed in 1985, and the extension of this technology to the laboratory microfusion regime is technically straightforward. We have recently completed a point design for such a solid-state laser for LMF, and we are quite confident that using technology at hand a full high-gain facility could be completed in about 10 years at a cost of about \$800 million in fiscal '89 dollars.

Now, in the absence of a change of national priorities such as, perhaps, caused by a comprehensive test ban, we judge, however, that the DOE defense program cannot justify a billion-dollar facility in as-spent dollars for weapons physics alone, and so we have begun to reassess higher technical risk approaches. We have recently completed the preliminary assessment of materials and technologies for a laser pump, solid-state laser, and we have just initiated a 4- to 5-year research and development program to assess this option, which, if successful, we feel has the potential to reduce the cost of this Laboratory Microfusion Facility by about a factor of 2 or to the cost of about \$400 million.

Now let me briefly turn my attention to ICF power plant technologies. Since that is outside the main mission of the defense program objectives, we have only looked at these issues at the discretionary level over the past many years and, consequently, we're not as far along as the magnetic fusion community in assessing these technologies. However, our studies indicate that the inertial fusion approach has a number of what we feel are attractive features. The first is what we call action-at-a-distance in that the fusion driver, the laser or particle beam, can in fact be completely separated from the fusion confinement chamber, and this allows most of the high technology equipment to be far removed from the fusion and neutron environment.

Another unique, and we feel also valuable, feature results from the possibility of using flowing renewable materials for the so-called first wall within the confinement chamber. This greatly reduces the amount of activation products and permits confinement chamber designs that can last the 30-year lifetime of the power plant. Now, you also, of course, you have to have a driver for this power plant and it must be, in addition to, of course, producing the several megajoules and 1,000 terawatts, it must be efficient, operate at a repetition rate of a few times per second, and cost less than 25 percent roughly of the total power plant cost.

There are several potential candidates including linear induction heavy ion accelerators that Steve Dean mentioned, now pursued primarily at Lawrence Berkeley Laboratory, and gas-cooled solid-state lasers, an extension of the work we're doing at Livermore. These are both very promising but none of them have been developed far enough that its capabilities can be confidently extrapolated to meet all the requirements.

I'd like to just close with some thoughts on the future of ICF. We think that we have made impressive technical progress in ICF in the last 2 years with the results and accomplishment that, in fact, some less than 5 years ago thought could not be achieved. The results obtained in the last 2 years in the Nova laser and the Halite/Centurion program provide, at least my confidence, that the ICF technique is scientifically feasible and that ultimately high gain would be achieved by a 5- to 10-megajoule laser.

Now, of course, some of this has not been lost on the international community. There is increasing ICF research and development activities worldwide. None of these activities are yet at the scale of the U.S. program. The Japanese program at Osaka University is the largest, with a civilian laser energy orientation. This program which includes collaboration with scientists from all around the world uses a glass laser that is the most powerful in the world after Livermore's Nova. Several other countries have established civilian research programs with smaller lasers. Next to the United States the largest programs for defense purposes are in France and England. Several countries without U.S. involvement are now exploring the establishment of multinational laboratories in which the pursuit of science and technology of ICF including the development of driver technologies compatible with power plant requirements. For example, in Italy—in Italy, there is an attempt and a proposal to build a large heavy-ion accelerator effort to pursue for ICF reactor concepts, and the European scientific community as a whole also recently proposed constructing a Nova-class laser facility.

Because of the commitment to the near-term defense mission of the United States ICF program, we do not formally interact with foreign ICF activities with two exceptions: a classified exchange with the United Kingdom and a limited unclassified collaboration with France. The United States still leads the world in ICF research and development and I believe we can lead the world in developing ICF as an energy technology. Clearly, advanced energy technology is an area where the U.S. must be second to none. I think it's important that the first fusion power plants, whether they be ICF or magnetic fusion, not be imported from abroad.

The most difficult problem to be faced now, at least speaking for inertial fusion, if you are to move ahead vigorously appear to be scientific—excuse me—not scientific or technical, but financial. But inertial and magnetic confinement fusion programs continue to demonstrate technical progress towards achieving the conditions that are required for electric power production. Now, the magnetic fusion program is constituted as an energy program and has rightly invested significant resources over the years in the science and technologies required for the production of electric power from fusion. By contrast, as I have told you, the ICF program is funded

principally as a defense program and has not had a comparable effort focused on the civilian energy requirements. Additional funding designated for the civilian aspects of ICF would allow these technologies to be studied and developed without diluting the ICF program's focus on its near-term defense mission.

With a national commitment, I believe that technology solutions for prototype fusion power plants could be in hand early in the first decade of the next century, by about 2010, and prototype fusion power plants by about 2020 to 2030. I agree with the other speakers about that time period. I am convinced that the goal of fusion as a commercial energy source is one of unprecedented importance for mankind. Your committee has understood that challenge and has supported the vision, and we of the fusion community profoundly appreciate that understanding and support.

Thank you.

[The prepared statement of Dr. Storm follows:]

**Civilian Applications of
Inertial Confinement Fusion:
Research, Development, and Demonstration**

**Statement prepared for
Subcommittee on
Energy Research and Development
House Committee on Science, Space, and Technology**

February 21, 1989

University of California
 **Lawrence Livermore
National Laboratory**

CIVILIAN APPLICATIONS OF
INERTIAL CONFINEMENT FUSION:
RESEARCH, DEVELOPMENT, AND DEMONSTRATION

STATEMENT PREPARED FOR
SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

HOUSE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

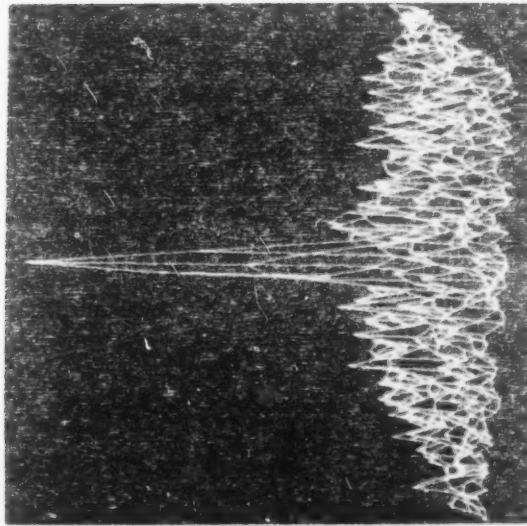
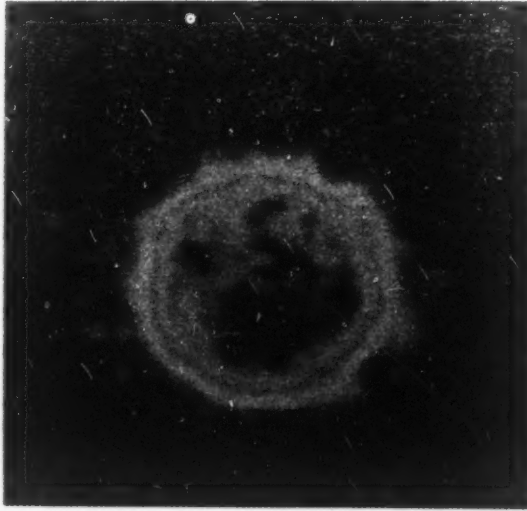
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BY

ERIK STORM
DEPUTY ASSOCIATE DIRECTOR FOR
INERTIAL CONFINEMENT FUSION
LAWRENCE LIVERMORE NATIONAL LABORATORY

FEBRUARY 21, 1989

Neutron image shows the burn region
of a high compression ICF target



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CIVILIAN APPLICATIONS OF INERTIAL CONFINEMENT FUSION: RESEARCH, DEVELOPMENT, AND DEMONSTRATION

FOREWORD

This is my first opportunity to appear before this Committee to provide my opinions on the promise of fusion—particularly Inertial Confinement Fusion (ICF). I want to express my appreciation to the Committee for this opportunity. There is increasing interest and activity in ICF research and development worldwide, motivated by the long-term civilian energy applications as well as, in some cases, the near-term military and defense missions. I am pleased to report that the United States' ICF Program has made significant technical progress since it was last reviewed for you two years ago. The United States continues to lead the world in ICF research and development, and I am particularly gratified that the Program at the Lawrence Livermore National Laboratory (LLNL) has continued to be at the forefront of that progress.

INTRODUCTION

Fusion has the potential to provide an environmentally attractive, inherently safe, and virtually inexhaustible energy source. Our awareness of the need for new energy sources that meet these requirements is becoming more acute because of the diminishing reserves of fossil fuels, rising worldwide energy demand, and increasing concerns about the environmental consequences of continuing with our present fossil fuel based energy technologies. I will summarize the recent technical progress that has given us even greater confidence than we had just two or three years ago that ICF is scientifically feasible. The new data support our projections about the basic feasibility of achieving ignition and high energy gain under the conditions required for electric power production with ICF. The rate of progress has motivated the Department of Energy to begin a planning activity for a Laboratory Microfusion Facility (LMF). That facility would

demonstrate the achievement of ignition and high gain in the laboratory, thereby demonstrating the scientific feasibility of ICF while providing the capability for the full spectrum of defense applications of ICF.

There is increasing ICF research and development activity worldwide. None of these activities are yet at the scale of the U.S. Program. The Japanese experimental program at Osaka University is the largest with a civilian (energy production) orientation. This program, which includes collaborations with scientists from other countries, uses a neodymium-doped glass laser that is the most powerful in the world after LLNL's Nova laser; its design is based on pre-Nova LLNL technology. Several other countries have established civilian research programs with smaller lasers. Still other countries are conducting theoretical and computational modeling of various aspects of ICF. Next to the United States, the largest programs for defense purposes are in France and England. Several countries, without U.S. involvement, are now exploring the establishment of multinational laboratories in which to pursue the science and technology of ICF, including the development of driver technologies compatible with power plant requirements. For example, the European scientific community has recently proposed constructing a Nova class laser facility. Because of the military/civilian nature of the United States' ICF Program, the commitment to the near-term defense mission and the concomitant national security concerns, we do not formally interact with foreign ICF activities with two exceptions, a classified exchange with the United Kingdom and a limited unclassified collaboration with France.

The United States leads the world in ICF research and development. Based on our success, I believe that it is important and not too early to establish a significant civilian increment to the ICF Program that would fund research and development on laser and particle beam driver technologies appropriate to ICF power plant requirements as well as the technologies required for the power plant itself. Because energy is an essential component of national security, and because it can take several decades to put these new technologies on line, it is advisable to begin this development. I believe we can lead the world in developing ICF as an energy technology.

ICF PROGRAM GOALS

The goal of the U.S. ICF Program, since its inception in the early 1970's, has been to fully develop inertial confinement fusion technology for both defense and civilian applications. The Livermore ICF Program continues to be fully committed to both objectives. The full realization of these applications requires a fusion output energy of several hundred to 1000 MJ, or 50 to 200 times greater than the energy provided by the laser or particle beam driver used to initiate the process; we call this requirement high gain. The achievement of high gain will have payoffs for this country in two areas vital to national security—nuclear defense and energy production.

The principal near-term goal of the Program, after we acquire the necessary additional data in the next few years, is to provide an LMF that will produce yields of about 100 to 1000 MJ for defense applications. Our current research efforts are therefore focused on acquiring the remaining data that supports achieving high-gain ICF in the laboratory at an acceptable cost. Demonstrating high gain in the laboratory will also establish the scientific feasibility of ICF as a candidate for electrical power production.

The availability of a microfusion capability in the laboratory would significantly augment an active, ongoing nuclear weapons design and testing program and would assist in assessing nuclear vulnerability and hardening. Some of these areas are already benefiting from current ICF capabilities, however, the full potential of ICF will be realized only when high gain is achieved in the laboratory. In this high-yield environment, energy densities in significant volumes and radiation fluxes over significant areas will be produced that are thousands of times greater and of more relevant fidelity than is possible with any other laboratory source. If further limits on underground nuclear tests are negotiated, and particularly in the event of a comprehensive test ban, such a capability would contribute significantly to retaining, at the national laboratories, the personnel that constitute our nuclear defense expertise and without whom we

could not support a viable nuclear deterrent. High-gain microfusion in the laboratory would also provide a unique means of studying matter under extreme conditions and would provide the U.S. scientific community with an unmatched ability to work in areas that have been identified by the National Academy of Sciences as frontier fields of physics.

The principal long-term goal of the Program is to develop and demonstrate the inertial confinement approach to fusion as a pure or hybrid technology for electric power production. (The "pure" approach is the one already described—the use of thermal energy produced by fusion reactions to generate electricity. In the "hybrid" approach, neutrons released in the fusion process are used to produce fuel for fission reactors.) Both of these ultimate applications of ICF require significantly more technology development than the defense applications. However, the potential national and global payoff is very high. Duplicating on earth the fusion process that generates the energy of the sun has the potential to provide a virtually inexhaustible, environmentally attractive, and inherently safe energy option. In the long term, in addition to power generation, ICF reactors and ICF technologies have potential applications for fissile fuel production, isotope production, and space propulsion.

The ICF approach to obtaining a high-gain fusion reaction is to compress and heat fuel composed of isotopes of hydrogen, contained in a small fusion pellet, to a density close to 20 times that of lead and a temperature of about 100,000,000°C. Such conditions exist only two other places in the universe—in hydrogen (thermonuclear) bombs and in stars. By imaging the fusion products emitted from miniature "stars" produced by the Nova laser facility at LLNL, we have demonstrated a fundamental performance requirement for ICF. The photograph on the frontispiece shows the fusion neutrons from a pellet compressed in volume by a factor of nearly 32,000. For a brief instant, the laser drove a microfusion pellet to a temperature and density exceeding those in the sun. Although it is a long way from this generation of Nova experiments to the ultimate application and full return on the investment in ICF, the recent progress has reinforced our confidence that the goals are attainable. This progress has also supported a decision

within the Department of Energy that the time has come to plan for the next step, a Laboratory Microfusion Facility with which to make high-gain ICF a reality.

MAJOR PROGRAM OBJECTIVES AND MILESTONES

There are three significant milestones on the road to developing ICF for commercial power production. These objectives, discussed below, are also presented graphically in Figure 1.

- The first milestone is to demonstrate scientific feasibility. We must develop the science and technology base for the target and the driver so we can confidently define the technical requirements for obtaining high gain, build a laboratory high-gain experimental facility (LMF), demonstrate high gain in the laboratory, and optimize the driver-target performance.
- The second milestone is to build an "engineering test reactor" to develop and demonstrate solutions to the technology and engineering requirements.
- The final milestone is to build a demonstration power plant and optimize the specific technologies for commercially competitive fusion power.

ICF has made excellent progress on target physics issues in the last few years. Experimental results have significantly increased our confidence about the basic feasibility of achieving high gain. It is now generally accepted that the driver energy required to achieve high gain is between 5 and 20 MJ. Economic considerations make the lower end of that range (5 to 10 MJ) highly desirable—and possibly necessary. It is my belief that the target physics data supports the projection that high gain can be achieved within that lower range of driver energies. The current funding-limited experimental program at LLNL is directed toward answering the remaining questions required to support the construction of an LMF with which to

complete the first milestone. If funding were restored to the equivalent of that in fiscal year 1985, we believe that a more comprehensive basis for a decision on the direction of the ICF Program can be available in about four years. The extremely rapid progress over the last two years has demonstrated that the experimental capabilities and theoretical modeling tools are in place to justify an accelerated program.

PROGRAM STATUS AND ACCOMPLISHMENTS

The national ICF Program is pursuing the demonstration of the scientific feasibility of high-gain ICF with a three-component program. In order to present program status and accomplishments, I will first briefly review the technical components of that program:

- A laboratory component to study driver-target performance with targets up to 1/10 full scale. This effort will provide the physics and technology base to quantify most of the requirements for achieving high gain in the laboratory. The LLNL Nova laser is the primary facility for this component.
- A classified component, Halite (at LLNL) and Centurion (at Los Alamos National Laboratory), utilizing underground nuclear tests at the Nevada Test Site. The purpose of this component is to study the design characteristics of efficient ICF targets. Halite and Centurion have played a vital role in establishing our confidence in the scientific feasibility of ICF.
- The driver development component, aimed at providing the technology base for a low-cost, flexible, high-energy, high-power driver to demonstrate and optimize high gain in the laboratory. A desirable but presently underfunded and underemphasized adjunct would be to identify an associated technology path to a driver with the characteristics required for an ICF power plant.

The centers for ICF research in the United States are at the Lawrence Livermore National Laboratory, the Los Alamos National Laboratory, and the Sandia National Laboratory. Supporting activities are carried out by an industrial participant, KMS Fusion, Inc., by the University of Rochester Laboratory for Laser Energetics, and by the Naval Research Laboratory.

Because of budgetary constraints and technical priorities the LLNL ICF Program has recently consolidated its efforts into a two-component program: target physics, with Nova, and driver development; the Halite program has been suspended.

Nova Experiments at LLNL

The LLNL Nova laser facility, a rendition of which is shown in Figure 2, is the primary U.S. facility devoted to the study of the indirect (or hohlraum) drive approach to inertial fusion. Nova's principal objective is to demonstrate that laser-driven hohlraums (hollow chambers that trap electromagnetic radiation) meet the conditions of beam/target coupling efficiency, beam irradiation symmetry, beam pulse shaping, target preheat, and hydrodynamic stability required for high gain. In the past two years, rapid progress has been made toward all these objectives. The first of these comprehensive indirect-drive experiments with Nova was conducted in 1986. In less than three years, in various experiments with about 20 kJ of 0.35 μ m light (less than 50% of Nova's full performance capability), values for the drive temperature and pressure, preheat, and symmetry that meet or exceed those required for high gain have been documented with a properly scaled target geometry.

Over the last year these tests have culminated in precision experiments (conducted in the Nova target chamber shown in Figure 3) that have demonstrated the successful implosions of capsules with a uniform volume compression exceeding the value of 30,000 required for the success of ICF. The photograph on the frontispiece of this testimony is an image produced by the fusion products emitted from a pellet

compressed by nearly a factor of 32,000 in volume. In other experiments volume compressions exceeding 50,000 were measured. This performance is a critical requirement for ICF, and its achievement meets a major program objective. The fuel pellet performance essentially matched optimal, one-dimensional computer predictions of implosion values. The experiments also demonstrated control of the hohlraum environment—in particular, the achievement of the radiation flux uniformity required for high gain. This is the first time that ICF theory and experiments have agreed so closely, and it indicates that our ability to predict and understand results has greatly improved. The experimental results were achieved both through improved theoretical understanding and computational modeling and through the use of ultraviolet (0.35- μm) light from the Nova laser. The physical conditions in these experiments were chosen to match those of appropriately scaled high-gain targets, and we achieved a combination of confinement time and fuel temperature that was less than a factor of 10 away from the value needed to meet the criterion for fusion ignition and high gain, as shown by the solid Nova point in Figure 4. This result is comparable to the best conditions obtained in magnetic confinement fusion experiments.

We have just completed improvements to Nova that will allow us to operate it routinely at its full design energy. With the benefit of experiments at these higher drive energies, and using the sophisticated temporal pulse shaping capability of Nova and the advances in diagnostic capability we have added to the facility, we will refine target designs and expect improvements in our results to the regimes indicated as open circles in Figure 4. The successful execution of these physics and implosion experiments will confirm that fuel pellets indirectly driven with a 5 to 10 MJ driver will achieve the conditions required for high gain.

Hallite/Centurion

As discussed above, the principal approach to inertial fusion in the United States involves the use of indirectly driven targets. In this concept, energy from a laboratory driver is converted to x rays that are

used to implode and heat the fusion fuel in an inertial fusion capsule. The ability to study and understand the performance of such capsules in the laboratory has been limited by the energy and power that can be provided with presently available lasers. In Halite/Centurion, a portion of the much greater energy from a nuclear device in underground explosions at the Nevada Test Site has been used to implode inertial fusion capsules, thereby extending the range of inertial fusion research. These experiments have produced excellent results, contributing considerably to our increased confidence in the basic feasibility of achieving high-gain ICF.

The combination of Nova and Halite/Centurion data and the recent development of cryogenic high-gain target fabrication technologies makes us sure that in the next three to five years we can obtain the data and demonstrate the technology necessary to resolve the remaining target issues. I believe that this accomplishment will reduce the uncertainty in the drive energy required for high gain from the current 5 to 20 MJ to the anticipated 5 to 10 MJ.

Driver Development

There are two essential aspects of ICF research and development for driver technology. The first of these is directed at the driver requirements for the high-gain LMF and is discussed below. The second addresses the development of a power plant driver optimized for efficiency and cost-effectiveness and designed to repetitively drive optimized fusion targets. This second effort will be briefly discussed in the following section on ICF power plants.

The driver issues associated with high-energy, high-gain experiments that would be conducted at the rate of a few per week in an LMF are associated almost exclusively with cost. Our experience and analysis supports the opinion that the extension of present short-wavelength solid state lasers to the energy and power regime required to achieve high gain is technically straightforward. The 100-kJ, 100-TW class Nova

laser has proven to be an extremely successful and flexible experimental facility. The design and construction of the 10-beam Nova laser required the development of numerous capabilities and technologies in which more than 1200 U.S. companies participated. The result, in addition to producing the most powerful and versatile laser research facility in the world, was to provide new capabilities and products for a number of U.S. companies. This gave them expanded markets and increased competitiveness. Nova required, for example, over one hundred very high quality mirrors and lenses up to 3-1/2 feet in diameter. At the time, low-cost production quantities of such optics were not available. Working with several U.S. manufacturers, we developed the techniques and helped provide the facilities to allow manufacture of the required components. As a result, these companies now have the world's leading capabilities to produce large, high-precision optical components, and in many cases the technologies have been significantly advanced and extended into new fields. Figure 5 illustrates three of these improvements.

Drawing on this body of scientific, engineering, and industrial expertise and experience, we have refined an LLNL point design for a 10-MJ, 1000-TW solid state laser driver for an LMF. The design uses compact, modular units to increase laser efficiency and reduce assembly time and manufacturing costs. The design parameters and component costs are based on our experience with Nova together with engineering estimates based on present production techniques and demonstrated laser technology improvements. As a result of this study we are confident that using technology we have at hand a high-gain fusion laboratory facility (LMF), including experimental capability, could be completed before 1998 and at a cost of approximately \$800 million in fiscal year 1989 dollars. The 10-fold increase in power required beyond Nova, as shown in Figure 6, is a small extension of the more than 100,000-fold improvement in high-energy laser performance that we have already achieved at LLNL.

Based on concerns that a cost in the range of \$800 million for an LMF is unacceptably high, barring a significant change in national priority, we have begun to reassess higher-technical-risk approaches to

solid state laser designs that offer promise for significant cost reductions. Utilizing our experience with laser technology development, we believe that a substantial reduction in the cost of a high-energy solid state laser for an LMF is possible given an appropriate R&D program to support this activity. We have recently completed the preliminary assessment of materials and technologies for a laser-pumped solid state laser. This is a departure in laser architecture and technologies from conventional flashlamp-pumped neodymium-doped glass lasers such as Nova. We have just initiated a four to five year R&D program to assess this option; if successful, it has the potential to reduce the cost of the LMF by a factor of two, to a total cost of about \$400 million in fiscal year 1989 dollars.

ICF POWER PLANTS

Working with the nuclear reactor design community, we have studied the details of producing electrical power using pure ICF (the technique already described) and using the "hybrid" concept, in which fusion neutrons are used to produce fuel for conventional fission reactors. These studies indicate that the inertial confinement approach to electric power production has a number of attractive features that offer the potential for high efficiency and excellent safety.

The power plant driver (the laser or particle-beam accelerator) can be physically separated from the fusion reaction chamber. The driver and chamber can therefore be optimized individually, and most of the high-technology, high-maintenance equipment can be far from the environment of the reaction chamber. A single driver could, in principle, serve several chambers if the beam were switched from one chamber to another.

An additional feature possible with an ICF reaction chamber is that the so-called first wall, which is directly exposed to the extreme environment produced by the fusion reaction, can have the form of a flowing, renewable material. This replenished first wall protects the chamber's structural materials from much of

the shock and radiation associated with the fusion reactions, acts as the heat transfer fluid, allows for easier maintenance, and reduces external shielding requirements. If the material contains lithium, the fusion neutrons interacting with the lithium-bearing material breed tritium to fuel subsequent fusion targets. This design approach greatly reduces the quantity of long-lived activation products and makes it possible to design a system in which the structural wall lasts for the 30 year lifetime of the plant.

We are continuing to refine our analyses of our Cascade reactor concept (Figure 7) which incorporates the features just described. Cascade has received the most detailed engineering and cost analyses of any proposed ICF reactor design. Earlier designs utilized liquid lithium, which presents a fire hazard. In Cascade the lithium is contained in ceramic granules that cannot burn. A layer about one meter thick, composed of these one millimeter diameter granules, flows along the wall of the reaction chamber as the entire chamber rotates. The granules can withstand high temperatures, so Cascade can achieve a power conversion efficiency of 55%, and an overall plant efficiency of 49%. The first structural wall consists of silicon carbide panels, which will have low residual activation.

Further studies must be conducted before Cascade's potential can be fully determined. For example, experiments must be conducted to ascertain the lifetime and manufacturing cost of the ceramic granules, which will affect the viability and economic competitiveness of Cascade. If such matters prove to have slight cost impact, Cascade would be cost-competitive with coal and with advanced fission reactors. Given the amount of study already devoted to Cascade, and the promise that this design appears to hold for a viable ICF power plant, it seems appropriate now to enhance studies of power-plant technologies, particularly studies of materials and of reaction-chamber design.

It is also appropriate to give considerably more emphasis to identifying an appropriate ICF power plant driver. Besides delivering a high-energy, high-power beam, such a driver must of course be efficient, must be capable of reliable operation over extended periods at a few pulses per second, and must cost less

than 25% of the total power plant cost. No candidate driver has been developed far enough that its capabilities can be extrapolated to meet all these requirements.

Flashlamp-pumped neodymium-doped glass laser amplifiers, such as those used in the Nova laser, can deliver the necessary peak energy and power, but not the required efficiency or repetition rate. It appears that the use of a flow-cooled crystalline amplifier medium, instead of glass, would permit us to meet both of these criteria as well. However, technical and economic challenges must be addressed and solved before this technology can be extrapolated to the required conditions.

Multistage linear-induction accelerators for heavy ions (such as lead) appear to be capable of the efficiency, repetition rate, and durability required in a power plant. Although technologically promising, work on the heavy-ion drivers, principally being conducted at Lawrence Berkeley Laboratory, has been funded at a relatively low level, and therefore they are not as far along the development path as that for other drivers.

THE FUTURE OF ICF

We think that we have made impressive technical progress in ICF in the last two to three years, with results and accomplishments that, less than a decade ago, some thought could not be achieved. The most difficult problems we now face if we are to move ahead vigorously appear to be not scientific or technical, but financial. Results obtained over the last three years with the Nova laser and in the Halite/Centurion program provide my confidence that the ICF technique is scientifically feasible.

Figure 8 shows (in fiscal year 1989 dollars) the funding history of the LLNL ICF Program. For some time the program operated at the equivalent of \$80 million/year in today's dollars; today our operating budget is \$64.3 million. This budget erosion has had a serious impact on our program. This year we have had to

impose a drastic curtailment in Nova experiments, severely reduce technology development efforts for advanced drivers, eliminate technology development for reactor drivers, and reduce work on power-plant concepts to paper studies. This has resulted in a recent staff reduction of 60 more people. With continued funding at this level, the LLNL ICF Program would of necessity become a one-component program within about three years, comprising Nova experiments only; we would probably have to abandon attempts to develop a low-cost, high-energy laser driver by the early 1990's, and all work on ICF energy-production technologies. Funding of the LLNL ICF Program at its historic level of \$80 million (fiscal year 1989 dollars) appears to me to be the minimum consistent with restoring a balanced program.

Further, our technical accomplishments lead me to believe that it is appropriate to introduce new funds to support more vigorous development of the civilian applications of ICF—in particular, ICF power plant drivers and other power plant technologies.

Both the inertial and magnetic confinement fusion programs continue to demonstrate technical progress toward achieving the conditions they require for electrical power production. The magnetic fusion program is constituted as an energy program. It has invested significant resources over the years in sciences and technologies required for the production of electrical power from fusion. By contrast the ICF Program is funded principally as a defense program and has not had a comparable effort focused on the civilian energy requirements. Additional funding designated for the civilian aspects of ICF would allow these technologies to be studied and developed without diluting the ICF Program's focus and commitment to its defense mission.

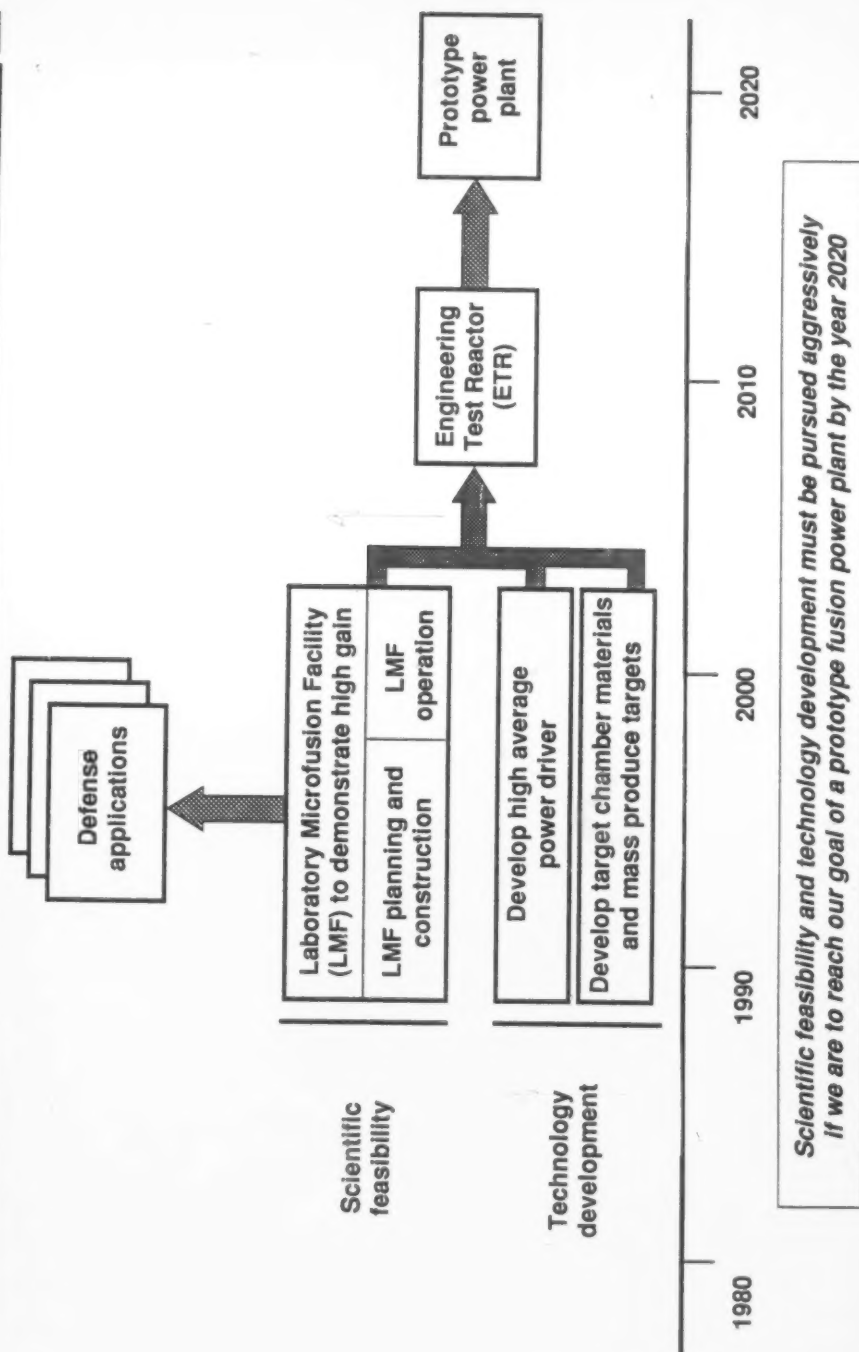
I believe strongly that energy science and technology is one of the most important areas for the United States in the long term. The signs of serious energy and environmental problems are becoming more visible at an increasing rate. We should move aggressively toward developing alternative sources that can begin to provide a significant fraction of our energy in 30 to 50 years. I believe that ICF should be one

of them. During the last few years there has developed broad based concern over the degree to which the United States is in danger of losing its lead in many high technology areas. Advanced energy technologies is an area in which I strongly believe the United States must be second to none.

Fusion works; the stars are our natural fusion power plants. An aggressive program to develop commercially competitive power plants is scientifically and technologically realistic. The question left for ICF is exactly what driver energy is required for an efficient system. With a national commitment I believe that the technology solutions for a prototype ICF power plant could be in hand by the first decade of the next century. I am convinced that the goal of fusion as a commercial energy source is one of unprecedented importance for humankind. I hope we have the vision and the will to accept the challenge of fusion.

Your Committee has understood the challenge and has supported the vision. We of the fusion community profoundly appreciate that understanding and support.

The objectives and goals for the defense and civilian applications of ICF are well defined



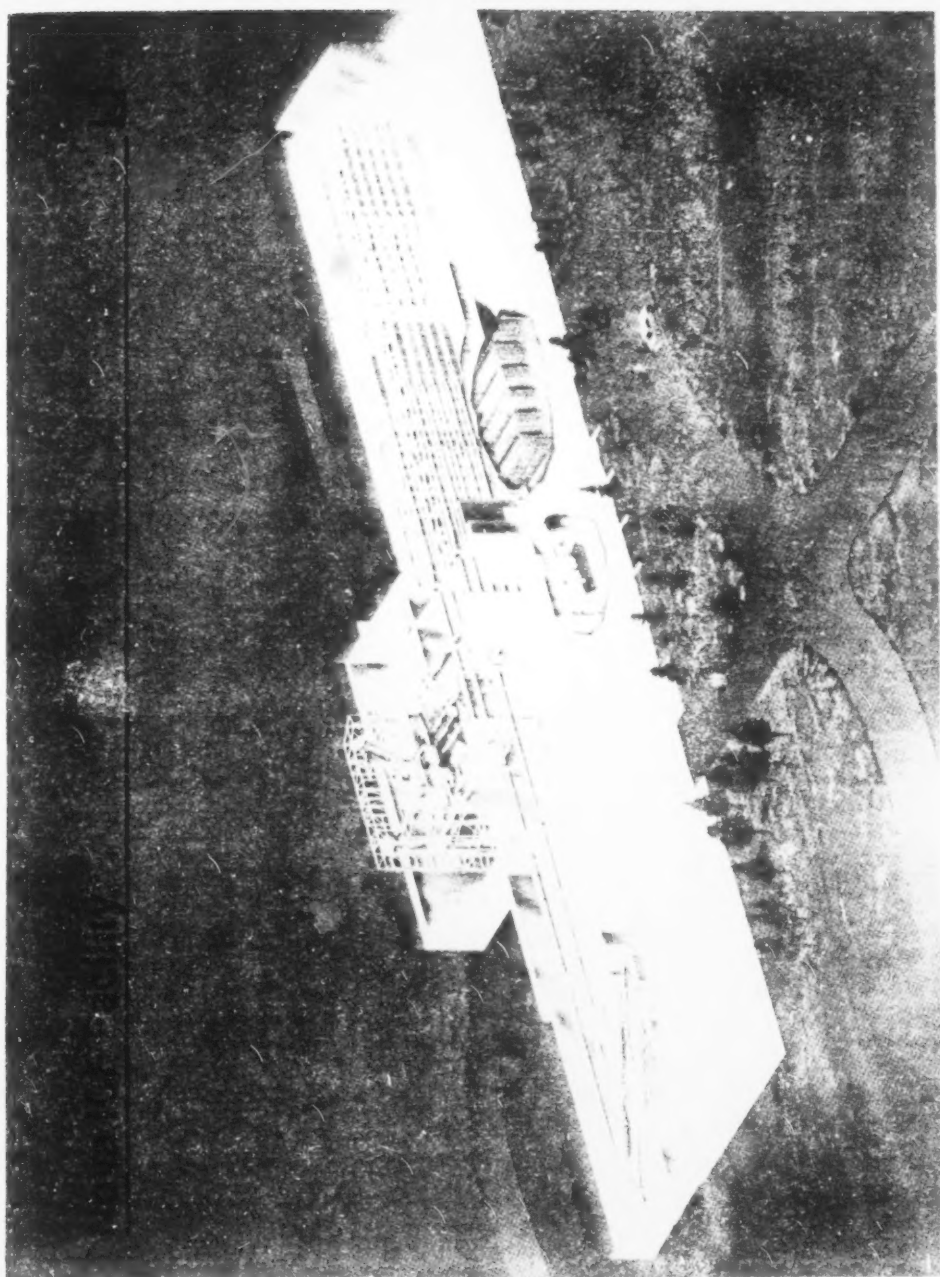


Figure 2

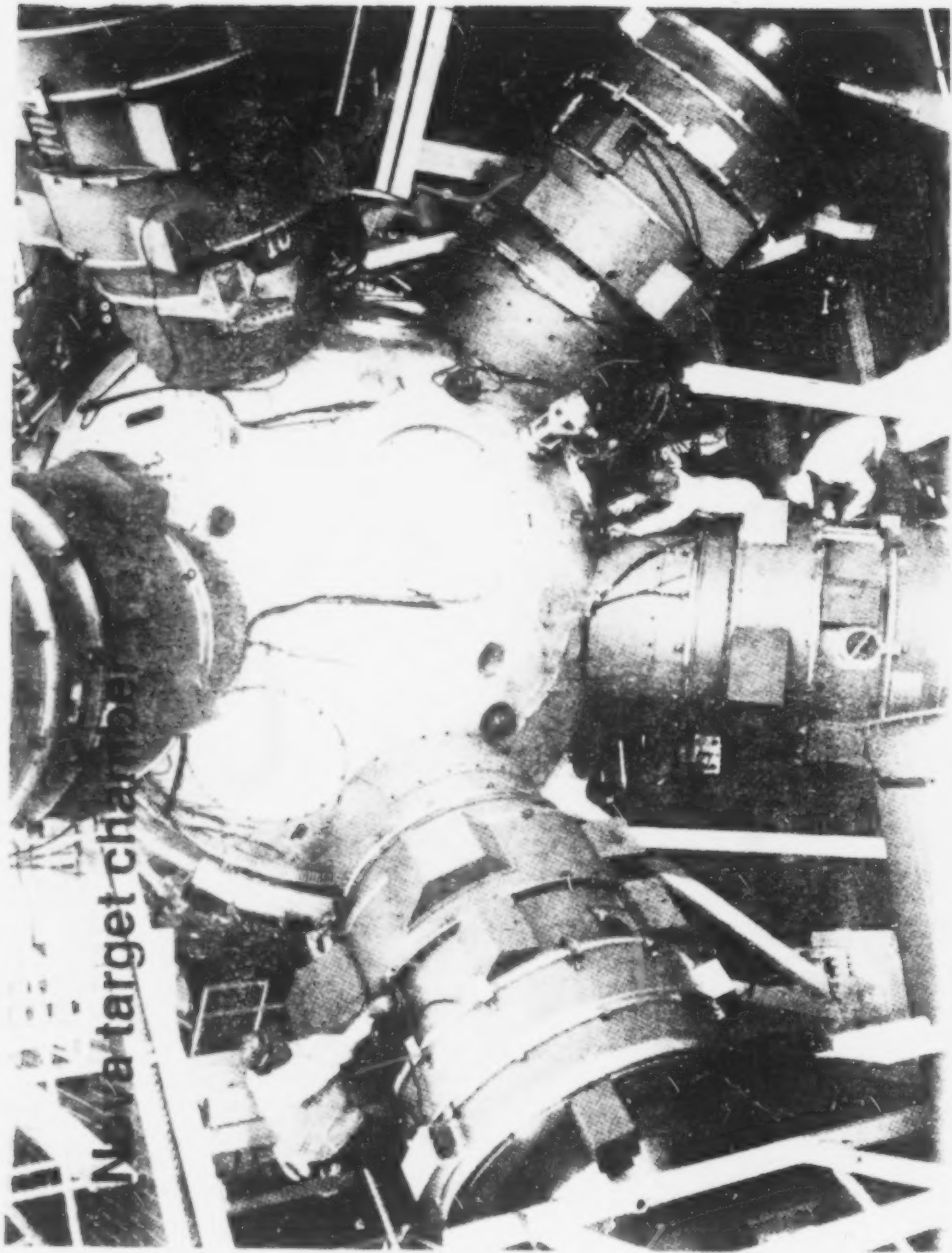
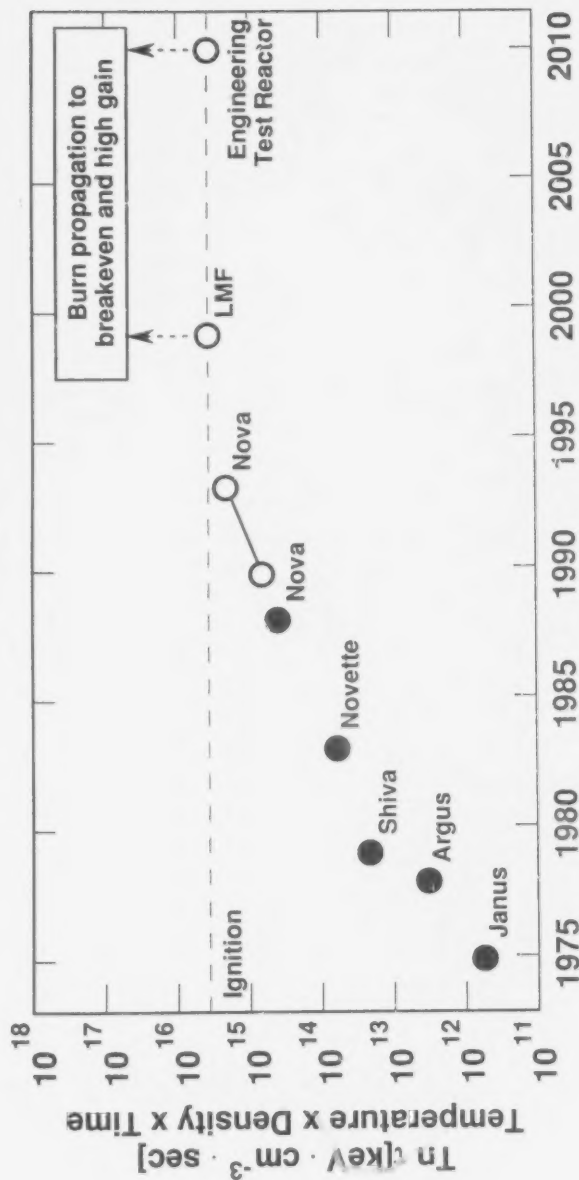


Figure 3

Inertial Confinement Fusion (ICF) progress in achieving conditions required for fusion power



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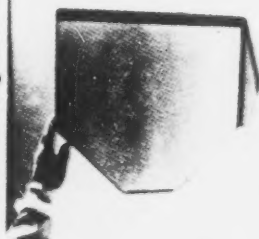
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Figure 4

Many improvements in laser and optics technology have been made since Nova



New laser glass



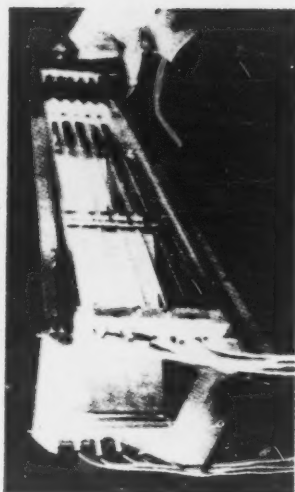
- Damage resistant
- Reduced cost

2.5-m aspheric generator
(Eastman Kodak)



- Computer controlled ductile grinding to 1μm accuracy

Improved amplifiers

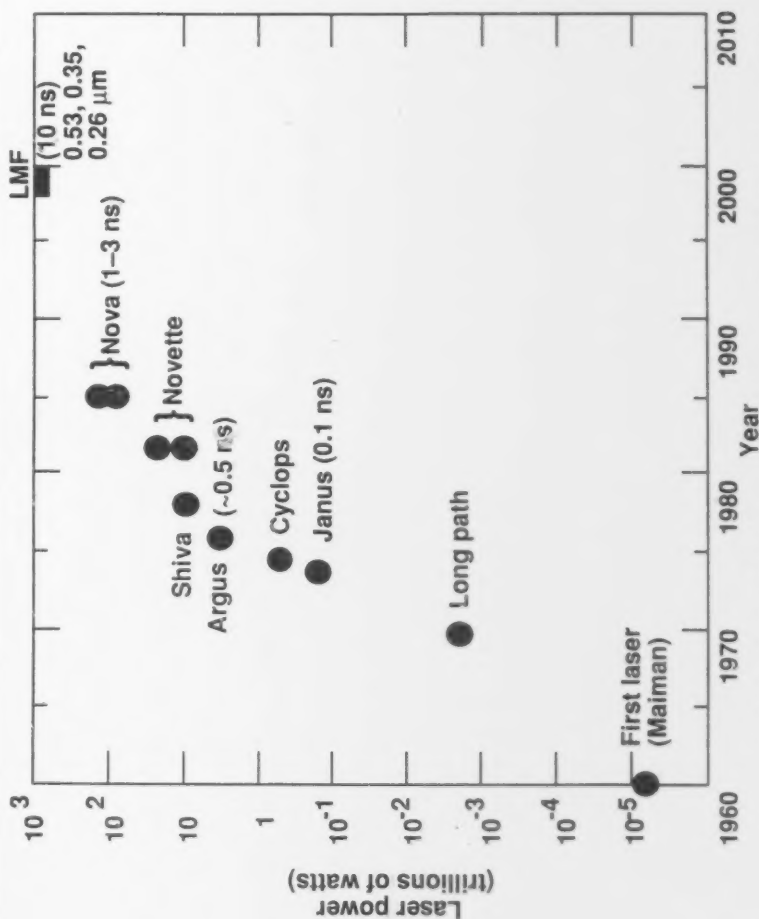


- 3× higher efficiency than Nova amplifiers

⇒ Improvements and economics-of-scale substantially reduce projected cost for a high-gain driver

Figure 5

LLNL solid state lasers are very close to the power needed for an LMF



It is technically assured that a multimegajoule, ~1000 TW solid state laser can be built

Figure 6

Cascade ICF Power Plant

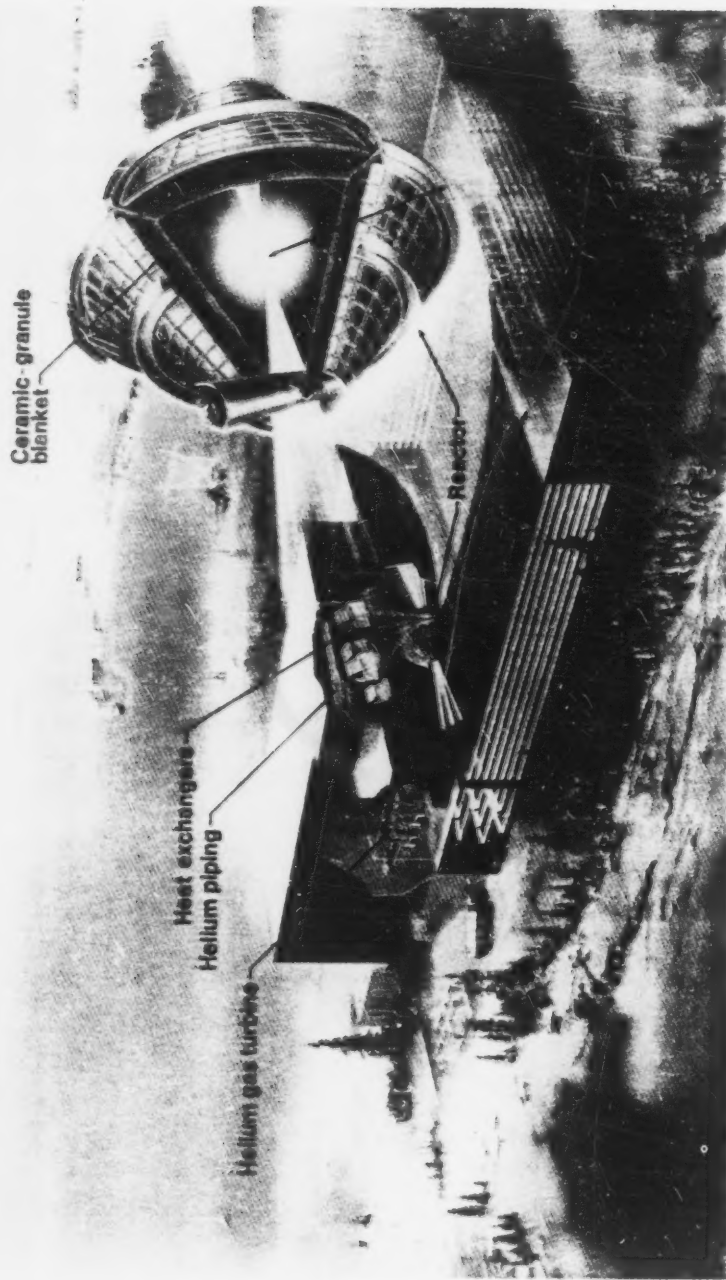
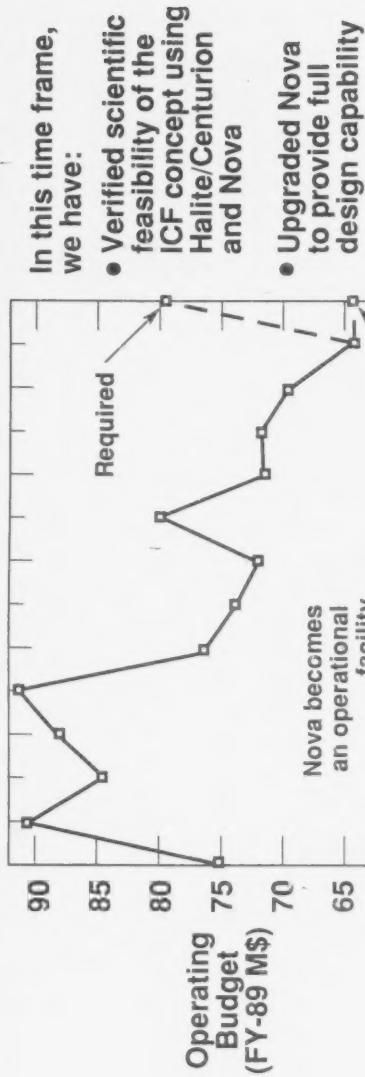


Figure 7

ICF operational funding continues to be well below the critical level for maintaining a balanced program



- The Required case productively exploits the progress made to date
- The Proposed case perpetuates a sub-critical program

\$80M is the minimum recommended level of operational funding

Figure 8

Mrs. LLOYD. Thank you very much, Dr. Storm, for your excellent testimony. Both of you are certainly great witnesses.

PANEL 2 DISCUSSION

Mrs. LLOYD. If our hearings today, the first hearings on our budget for next year are any indication of what's to come, then our budget hearings promise to be a very painful process.

As you know, the members of this committee have been very supportive of the fusion program and we've suffered with you as we've seen the decline in the funding of the programs, Dr. Dean. It occurred to me while listening to your testimony, thinking what the European Community is doing, that perhaps if more of our allies spent more of their R&D dollars on civilian R&D and—spent more on defense R&D as we're doing, maybe we'd have a little bit more money left for civilian R&D as well. I think we need a smattering of burden sharing in this regard.

Dr. Dean, how much do the Soviets spend on their fusion program?

Dr. DEAN. It's never been able to be calculated, in part because we don't really know what a ruble buys. But we know that they have about twice as many people working on fusion as we have, although when we assess the capability of their facilities we feel that their capabilities is actually somewhat less than our capability and so that their output in terms of actual experimental results anyway is somewhat less than ours, although their theoretical effort is very strong. On the whole my own impression is that the Soviet effort is in some sense comparable to ours. They have more people but their equipment is not as good, and it kind of balances out to where their progress is more or less as good as ours, maybe a little bit behind.

Mrs. LLOYD. Is American industry having to struggle to keep up with international competition?

Dr. DEAN. I'm not sure, except for General Atomics, which is certainly an important part of the U.S. fusion program, that there is any U.S. industry in fusion anymore. I think one of the casualties of the last decade has been that with the absence of construction and the decline of the budget that there is almost no U.S. industry participation on a regular basis now in the U.S. fusion program, in contrast to the situation in Japan where there are strong in-house fusion efforts in major companies like Hitachi and Toshiba and Mitsubishi.

Mrs. LLOYD. What private firms are involved in CIT, and how have they participated?

Dr. DEAN. In the CIT?

Mrs. LLOYD. Um-hum.

Dr. DEAN. You'd have to really ask Princeton for the details, you know. Harold Furth indicated that in this latest reprogramming they laid off 160 contract personnel. I know that two of the major companies that were hurt by that were Burns & Rowe and Ebasco Services. Probably others. And, if CIT construction doesn't go forward, then I think some of the other companies that were about to come in like McDonnell Douglas will also be hurt by that slippage.

Mrs. LLOYD. We're becoming more and more dependent on foreign know-how then, are we not?

Dr. DEAN. Well, you know, I think in the best of all worlds the U.S. would be developing fusion technology in industry, and industry would be involved in building facilities. And in the absence of building facilities and with the decline of our technology programs, there isn't very much for industry to do. So that when and if we come back to building facilities and go out on bid it is going to be very hard for the U.S. industries to come in with competitive bids if companies like Hitachi and Toshiba and Mitsubishi are in fact in the bidding.

Mrs. LLOYD. Thank you very much.

Dr. STORM. How much of the ICF effort is being directed toward civilian applications? Are these applications more difficult or less difficult would you say?

Dr. STORM. Let me see, I think your first question was how much of the ICF budget is spent on—

Mrs. LLOYD. How much of the work. How much of the effort is really directed toward your civilian applications?

Dr. STORM. As of right now there is, over the past many years we had spent at the discretionary level developing technologies for inertial fusion power, and that has been a very low level. This year, speaking for Livermore, we're down to about half a million dollars on some reactor technologies, and that's mainly paper studies, and about \$1 million on lasers, drivers for that. So it's truly at almost no effort anymore.

Mrs. LLOYD. How much DOE defense money did you receive last year for ICF?

Dr. STORM. At Livermore the budget for ICF under the defense program was \$64.3 million. The national ICF budget was about \$155 million.

Mrs. LLOYD. One hundred and fifty-five million?

Dr. STORM. National level.

Mrs. LLOYD. For ICF?

Dr. STORM. Yes.

Mrs. LLOYD. Okay. What is the proposed budget for this year, are you aware?

Dr. STORM. I think it's a hundred and—it's up 3 percent to about \$160 million.

Mrs. LLOYD. So there is an increase in the Department of Defense, DOD money.

Dr. STORM. They've kept it—yes, level with inflation. That's correct.

Mrs. LLOYD. So your budget is growing?

Dr. STORM. We haven't seen the breakout yet for Livermore specifically, but I would hope it will at least keep up with inflation.

Mrs. LLOYD. How does ICF compare with magnetic fusion from a materials point of view?

Dr. STORM. As I said, we feel that the ICF concept offers some attractive features for a power plant. There are some drawbacks in that it's post-repetitive environment. I think it should be at this point viewed as an interesting and attractive alternative option for fusion power, something used—the recent result in physics have

put it to the point where it should be pursued, we feel, as an equal partner in the energy option for the future.

Mrs. LLOYD. Thank you very much.

Mr. Morrison?

Mr. MORRISON. Dr. Dean, you've been at this for a long time. There may be a very significant question as we, as the game keeps changing here. Is—how much environmental interest is there in helping us develop electrical energy from fusion, and has the recent discussion on global warming spun off to push us in this direction?

Dr. DEAN. Yes. We've been having, our association has been having some more frequent interaction with the environmental community. I would say that they have not yet really focused their minds on things like fusion in part because fusion to them is not yet a real energy option. So, you know, they're starting to think about the global warming issue in the context of what the near-term energy mix can be on, in our energy system and not on what the energy system might look like 50 years from now. So I would say there's starting to be an awareness in the environmental community and an interest in fusion, but because they're more near-term focus rather than long-term focus and they don't know whether fusion is really a feasible option, they're certainly not coming out strongly advocating the development of something like fusion, yet although we're working with them and hoping that we can bring them around to that point of view.

Mr. MORRISON. Be sort of interesting to see what might be done with getting some people together. Of course, their major thrust is conservation, absolutely convinced that we can solve the world's problems with conservation. I think we can get part way there, but I would see, you know, the answer being conservation, nuclear power in a variety of forms and—I mean, fission power and then fusion power. And they might just be able to help us get the investment which you initially saw back in the 1970s that led to the Fusion Energy Act which you were very active in and my predecessor, Mike McCormick, was very much involved with. I, for one, would love to help you with that effort.

Dr. DEAN. Yes. Well, we'd appreciate your help. Our annual meeting this year, which we're just scheduling for June, is going to be on Fusion and the Environment, and we're planning to bring into that meeting spokespeople for these environmental groups and try to generate this connection more strongly between fusion and the environmental interests.

Mr. MORRISON. All right. Dr. Storm, just one question. Maybe after being on the Red Eye all night and then sitting through this hearing my mind is leaving me. But is there a potential with your approach to creating the fusion reaction that we would build a box to contain the reaction and you could come around with your kick-starter and start them up and then move on to the next one?

Dr. STORM. Ah, not quite but in a sense. I mean, one of the possibilities this action-at-a-distance for ICF allows you is to have several confinement chambers shared by a common driver. That's not, perhaps, quite what you had in mind, but it's a possibility you could have several smaller units, then they would be all driven by a common driver as you can multiplex between them. But you

couldn't just kick start it and leave it running. You have to inject a pellet once or twice a second and fire a laser again each time the pellet is injected into this chamber. So, you have to kick start it, if you want, twice a second to keep it running.

Mr. MORRISON. I was trying to make it easier than that—

Dr. STORM. No.

Mr. MORRISON [continuing]. But I guess it's not.

Dr. STORM. But it is in the sense that, er, there's something is. That one driver could be common to several fusion confinement chambers, and that allows you flexibility.

Mr. MORRISON. Okay. And there's so far yet in thinking there is no mechanism for continuing to feed fuel to it and have it sustain its own reaction?

Dr. STORM. Well, in the sense, you know, you would have a—the inertial fusion confinement approach for a power plant would be you have this large chamber, sort of 5 meters radius, and you inject ballistically a small ICF pellet about once a second, and then you hit it with the laser, make this miniature star, collect the energy, and you just keep doing it. In the same way as when you run your internal combustion engine. It feels like it's running continuously but you're really firing the pistons several times a second.

Mr. MORRISON. And that's worked rather well as a concept.

Dr. STORM. Yes.

Mr. MORRISON. Thank you, Madam Chairman.

Mrs. LLOYD. Mr. Schiff?

Mr. SCHIFF. Thank you, Madam Chair. I'll be brief.

Dr. Storm, one question, sir. You testified about America keeping its leadership in fusion research, and I believe you specifically said that we should not import fusion technology. We should try to develop our own. Earlier I was given to understand that this is not classified research, that this is shared. That this country is working not only with our allies, but with the Soviet Union.

If, in fact, fusion research is one big happy family, what then is the priority on the United States maintaining leadership and not importing other technology?

Dr. STORM. Okay. There's a major difference between the magnetic and the inertial fusion approach in that inertial fusion its near-term primary mission is for defense application.

Mr. SCHIFF. Sorry. I didn't understand. You were referring to inertial fusion specifically.

Dr. STORM. Yes. Right.

Mr. SCHIFF. I understand now. Thank you. I didn't. I thought you were talking about fusion in general.

Dr. STORM. No. I was—

Mr. SCHIFF. Okay. Thank you, Dr. Storm.

Dr. Dean, it may be because I'm new I may be asking an unfair question here, and I'll stand rebuked if I am. But you made an eloquent presentation to significantly bolster the funding in saying we'll get fusion as fast as we choose to pay for it and that we should bite the bullet. The problem is we don't bite the bullet here. We bite someone else's bullet so to speak. And suppose that an increase in, significant increase in fusion research was at the cost of, say, not building a manned space station or not funding the super-

conducting semicollider, would you consider fusion research above those two in priorities?

Dr. DEAN. Well, speaking for myself I do. But speaking for the country, that's your problem.

Mr. SCHIFF. I understand you'll get safe conduct out of the building too upon answering that. [Laughter.]

Dr. DEAN. I don't set the national priorities, you know. I'm just an ordinary citizen. My field is fusion and I feel that we can get the job done for you whenever you're ready to go. Competing with these other things is a difficult problem for people like yourselves to wrestle with, and you've always done a good job in the past, and, you know, we look forward to you doing a good job in the future.

Mr. SCHIFF. But I understand you to testify we've not done a good job in the past and that the funding for fusion has not been what you feel the level should be. Is that right?

Dr. DEAN. Well. But I think you've done a great job overall when you consider the other problems that you've had to deal with. I mean fusion, as I say in my testimony, is not the only program that suffered in the '80s. Most technology programs have suffered as much or more than fusion. And I think you all have done a wonderful job for fusion keeping us up to the level that we're at. You know, even though we've gone down by 50 percent, my heart bleeds for some of the other technologies that have almost been wiped off the map. You know, I don't think that that's right either.

Mr. SCHIFF. Well, my apologies if I asked an unfair question here. It is, ultimately.

Mrs. LLOYD. Mr. Schiff, we're not the bad guys.

Mr. SCHIFF. Thank you.

Dr. DEAN. I'd like to make one comment about your question to Erik about industry. It's true that in the scientific phase that we share all this information, but we are also developing technologies not only for commercial fusion but for other spin-off commercial applications. And, if all these technologies are available from Japanese and European industries and not available in the U.S., regardless of the fact that this information is shared and open it will not accrue to the benefit of the U.S. economy if we leave the U.S. industry out.

Mr. SCHIFF. Thank you, Madam Chair.

Mrs. LLOYD. Thank you, Mr. Schiff. Thank you for being with us. If there are no further comments, then the committee will stand adjourned until the morning at 9 o'clock when we'll resume our hearings, and we'll focus in on fossil energy in the morning. Thank you very much.

[Whereupon, at 5:07 p.m., the subcommittee was recessed, to reconvene at 9 a.m., Wednesday, February 22, 1989.]

APPENDIX 1



UNITED STATES ACTIVITIES

Promoting Career and Technology Policy Interests of Electrical, Electronics & Computer Engineers

April 3, 1989

United States Activities Board

Technology Activities Council
Aerospace R&D
Committee on Communications &
Information Policy
Committee on Man & Radiation
Defense R&D
Energy
Engineering R&D
Health Care Engineering Policy
Technology Issues
U.S. Competitiveness

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Technology Policy Conference
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The Honorable Marilyn Lloyd
Chairwoman,
Subcommittee on Energy Research and
Development
House Committee on Science, Space and
Technology
Room B-374 Rayburn House Office Building
Washington, DC 20515

Dear Congresswoman Lloyd:

Enclosed for your hearing record is the statement of the Institute of Electrical and Electronics Engineers, Inc., on the Fiscal Year 1990 budget for the Department of Energy's Magnetic Fusion Energy research program. Should you have any further questions regarding our perspectives on this program, we would be more than happy to provide you with additional supportive information.

Thank you for the opportunity to provide our views on this important research effort.

Sincerely,

William R. Tackaberry,
Chairman,
USA Technology Activities
Council

Enclosure

IEEE-USA, 1111 - 19th Street, N.W. Suite 608, Washington, DC 20036-3690

The Institute of Electrical and Electronics Engineers, Inc.

Statement To The House Committee on Science, Space and Technology,
 Subcommittee on Energy Research and Development
 by the
 U.S.A. Energy Committee
 of the
 Institute of Electrical and Electronics Engineers, Inc.
 on the Magnetic Fusion Energy Program
 of the U.S. Department of Energy
 April 3, 1989

The IEEE supports a strong stable government-funded program in fusion research, aimed at the establishment of the technical basis for the timely assessment of fusion as a long-range inexhaustible energy alternative with attractive environmental properties.

Among the nuclear alternatives, fission power is the most developed technologically, but has not yet been executed in an adequately reliable, safe manner; as such, it affords the most accessible short-term alternative to fossil fuels for power production, but must address environmental concerns. Fusion is further from commercialization, but has a greater potential for being environmentally more attractive.

Programs in both magnetic and inertial confinement fusion are underway and recently have made significant progress.

Magnetic Fusion Energy

Achievement of the goal of producing a timely fusion alternative demands a better supported, more stable long-term plan for the U.S. magnetic fusion program. The Magnetic Fusion Engineering Act of 1980 stated that a fusion reactor prototype should be operational by the year 2000; achievement of this goal required a steady increase in funding. In contrast, the U.S. Magnetic Fusion Energy (MFE) program is in decline and is unstable (with programmatic changes occurring more rapidly than projects can be constructed and satisfy goals), and the achievement of the legislated goal by the year 2000 is impossible. Since 1980, the fusion budget has been reduced to about 50% of its 1980 level.

Current projects such as the Advanced Torroidal Facility (ATF) at Oak Ridge, Deuterium Tritium (DT) operation in Tokamak Fusion Test Reactor (TFTR) at Princeton, the Microwave Tokamak Experiment (MTX) at Livermore and construction of the Compact Ignition Tokamak (CIT) at Princeton and the Confinement Physics Research Facility (CPRF) at Los Alamos have been significantly cut-back; staff have been laid-off from the fusion program as a result of the reductions. The Princeton Beta Experiment (PBX-M) tokamak at Princeton, aimed at the demonstration of existence of a regime of improved stability and confinement, is scheduled to be mothballed in FY 1990-91, with operations to be resumed in FY92.

Despite significantly less funding, the U.S. magnetic fusion program is nonetheless internationally competitive. As a measure of the relative paucity of funding of the U.S. program, the annual budget of the flagship device in the U.S. program is much less than its counterparts in Europe and Japan (the U.S.'s TFTR being at \$70M/year, the European JET (Joint European Tokamak) at \$120M/year, and the Japanese JT-60 at \$140M/year). Nonetheless, the U.S. fusion program continues to compete and contribute in the international arena. The TFTR at Princeton has produced plasmas of 30 keV (330 million degrees Celcius), has met or exceeded its original minimum design goal for the product of density, temperature and confinement time (advancing over a factor of 1000 beyond the value 10 years ago and being within a factor of 10 of the value required for a self-sustaining reaction), and is producing plasmas near "scientific breakeven." DIII-D at General Atomics in La Jolla has achieved a world-record beta (ratio of particle pressure to magnetic field pressure) of 8%. Concomitantly, physics understanding is improving.

For optimum study of critical physics issues and encouragement of necessary innovation, the near-term magnetic fusion program should include both operation

of existing devices, especially with modest upgrades for heating and diagnostic improvements, and construction of a new forefront device. The U.S. program has proposed a new high-field Compact Ignition Tokamak, targeted at the achievement of a self-sustaining fusion reaction. A device of this general type should be built, with a corresponding increase in the total Magnetic Fusion budget.

Complementing the physics advances, a balanced program must address the technological issues. The pursuit of the Engineering Test Reactor (ETR) will be so demanding (several billion dollars), that an international partnership such as the ITER team (involving the U.S., Japan, Europe and the USSR) is an appropriate approach for the design, development and operation of this class of device.

A strong domestic physics and engineering program is required to support such international collaborations, to stimulate innovation, and to sustain the required R&D diversity. In fact, the International Thermonuclear Experimental Reactor (ITER) program itself has developed a list of R&D issues to be addressed on existing devices. The U.S. must remain a stable contributing international partner, providing not only its share of the direct ITER support, but also executing a domestic program of international stature.

Inertial Confinement Program

In the Inertial Confinement Fusion (ICF) program, technical progress has included the demonstration that a driver energy of 5 to 20 MJ is adequate to achieve high gain. The results of the next 3 to 5 years' research will examine the feasibility of adequate gain at the lower end of this energy range.

Internationally, the U.S. leads the world in ICF R&D. The Japanese have the largest civilian energy-production program. France and England have large programs for defense purposes. Due to the military applications of ICF, the

U.S. does not engage in international collaborations on the same scale as in the magnetic confinement program.

Recommendation

The United States must develop a consistent energy policy which includes the timely assessment of fusion energy as an alternate source of electric power production with both a virtually inexhaustible supply of fuel and attractive environmental properties.

The fusion program should include both a strong domestic program and an international component for otherwise prohibitively demanding projects to achieve feasibility and timeliness. The domestic program should be targeted at resolving critical physics and technological issues and should include an optimized mix of operation of existing facilities and construction of relevant new facilities. The program plan should be adequately stable to permit the timely completion of construction projects, the efficient execution of research programs, the encouragement and exploitation of innovation, and the retention and development of technical staff.

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